Vol. 41, No. 1 January, 1934

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

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PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

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PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY

VOL. 41, NO. 1 JANUARY, 1934

THE PSYCHOLOGICAL REVIEW

THE VECTORS OF MIND[1]

BY L. L. THURSTONE

The University of Chicago

Under the title of this address, 'The Vectors of Mind,' I shall discuss one of the oldest of psychological problems with the aid of some new analytical methods. I am referring to the old problem of classifying the temperaments and personality types and the more recent problem of isolating the different mental abilities.

Until very recently the only attempt to solve this problem in a quantitative way seems to have been the work of Professor Spearman and his students. Spearman has formulated methods for dealing with the simplest case, in which all of the variables that enter into a particular study can be regarded as having only one factor in common. The factor theory that I shall describe starts without this limitation, in that I shall make no restriction as to the number of factors that are involved in any particular problem. The resulting factor theorems are quite different in form and in their underlying assumptions, but it is of interest to discover that they are consistent with Spearman's factor theory, which turns out to be a special case of the present general factor theory.

In this paper I shall first review the single-factor theory of Spearman. Then I shall describe a general factor theory. Those who have only a casual interest in the theoretical aspects of this problem will be more interested perhaps in the applications of the new factor theory to a number of psycho-

[1] Address of the president before the American Psychological Association, Chicago meeting, September, 1933.

logical problems. These psychological applications will constitute the major part of this paper.

It is thirty years ago that Spearman introduced his single-factor method and the hypothesis that intelligence is a central and general factor among the mental abilities. The literature on this subject of factor analysis has tended temporarily to obscure his contribution, because the controversies about it have frequently been staged about rather trivial or even irrelevant matters. Professor Spearman deserves much credit for initiating the factor problem and for his significant contribution toward its solution, even though his formulation is inadequate for the multi-dimensionality of the mental abilities.

Spearman's theory has been called a two-factor method or theory. The two factors involved in it are, first, a general factor common to all of the tests or variables, and second, a factor that is specific for each test or variable. It is less ambiguous to refer to this method as a single-factor method, because it deals with only one common or general factor. If there are five tests with a single common factor and a specific for each test, then the method involves the assumption of one common and five specific factors, or six factors in all. We shall refer to his method less ambiguously as a single-factor method.

We must distinguish between Spearman's method of analyzing the intercorrelations of a set of variables for a single common factor and his theory that intelligence is such a common factor which he calls '*g*.' If we start with a given table of intercorrelations it is possible by Spearman's method, and also by other methods, to investigate whether the given coefficients *can* be described in terms of a single common factor plus specifics and chance errors. If the answer is in the affirmative, then we *can* describe the correlations as the effect of (1) a common factor, (2) a factor specific to each test, and (3) chance errors. In factor theory, the last two are combined because they are both unique to each test. Hence the analysis yields a summation of a common factor and a factor unique to each test. About this aspect of the single-factor method there should be no debate, because it is straight and simple logic.

But there can be debate as to whether we should describe the tests by a single factor even though one factor is sufficient. It is in a sense an epistomological issue. Even though a set of intercorrelations *can* be described in terms of a single factor, it is possible, if you like, to describe the same correlations in terms of two or three or ten or any number of factors.

The situation is analogous to a similar problem in physical science. If a particle moves, we designate the movement by an arrow-head, a vector, in the direction of motion, but if it suits our convenience we put two arrowheads or more so that the observed motion may be expressed in terms that we have already been thinking about, such as the x, y, and z axes. Whether an observed acceleration is to be described in terms of one force, or two forces, or three forces, that are parallel to the x, y, and z axes, is entirely a matter of convenience for us. In exactly the same manner we may postulate two or more factors in a correlation problem instead of one, even when one factor would be sufficient. To ask whether there 'really' are several factors when one is sufficient, is as indeterminate as to ask how many accelerations there 'really' are that cause a particle to move. If the situation is such that one factor is not adequate while two factors would be adequate, then we may think of two factors, but we may state the problem in terms of more than two factors if our habits or the immediate context makes that more convenient.

Spearman believes that intelligence can be thought of as a factor that is common to all the activities that are usually called intelligent. The best evidence for a conspicuous and central intellective factor is that if you make a list of stunts, as varied as you please, which all satisfy the common sense criterion that the subjects must be smart, clever, intelligent, to do the stunts well, and that good performance does not depend primarily upon muscular strength or skill or upon other non-intellectual powers, then the inter-stunt correlations will all be positive. It is quite difficult to find a pair of stunts, both of which call for what would be called intelligence, as judged by common sense, which have a negative correlation. This is really all that is necessary to prove that what is

generally called intelligence can be regarded as a factor that is conspicuously common to a very wide variety of activities. Spearman's hypothesis, that it is some sort of energy, is not crucial to the hypothesis that it is a common factor in intellectual activities.

There is a frequently discussed difficulty about which more has been written than necessary. It has been customary to postulate a single common factor (Spearman's '*g*') and to make the additional but unnecessary assumption that there must be nothing else that is common to any pair of tests. Then the tetrad criterion is applied and it usually happens that a pair of tests in the battery has something else in common besides the most conspicuous single common factor. For example, two of the tests may have in common the ability to write fast, facility with geometrical figures, or a large vocabulary. Then the tetrad criterion is not satisfied and the conclusion is usually one of two kinds, depending on which side of the fence the investigator is on. If the investigator is out to prove '*g*,' then he concludes that the tests are bad because it is supposed to be bad to have tests that measure more than one factor! If the investigator is out to disprove '*g*', then he shows that the tetrads do not vanish and that therefore there is no '*g*.' Neither conclusion is correct. The correct conclusion is that more than one general factor must be postulated in order to account for the intercorrelations, and that one of these general factors may still be what we should call intelligence. But a technique for multiple factor analysis has not been available and consequently we have been stumbling around with 'group factors' as the trouble-making factors have been called. A group factor is one that is common to two or more of the tests but not to all of them. I use the term common factor for all factors that extend to two or more of the variables. We see therefore that Spearman's criterion, limited as it is to a single common factor, is not adequate for proving or disproving his own hypothesis that there is a conspicuous factor that is common to all intelligence tests. If his criterion gives a negative answer it simply means that the correlations require more than one common factor. We do

not need any factor methods at all to prove that a common factor of intelligence is a legitimate postulate. It is proved by the fact that all intelligence tests are positively correlated.

There is only one limited problem for which Spearman's method is adequate, namely, the question whether a single factor is sufficient to account for the intercorrelations of a set of tests. The usual answer is negative. His criterion that the tetrads shall vanish is rarely satisfied in practice. One might wonder then why it is that numerous examples have been compiled in which the tetrad criterion is satisfied. The reason is simply this—that in order to satisfy the criterion, the tests must be carefully selected so as to have only one thing in common. Another way by which the criterion is satisfied is to throw out of the battery those tests which do not agree with the criterion. The remaining set will then satisfy it. The reason for these difficulties is that Spearman's tetrad difference criterion demands more than his own hypothesis requires. His hypothesis does not state that there shall be only one common factor or ability. He himself deals with many factors. But his tetrad difference criterion requires that there shall be only one common factor.

Now it happens that one can readily put together several batteries of tests such that *within each battery* the criterion is satisfied and therefore we have a common factor '*g*' in each battery. But if we take a few tests from each of these batteries and put together a new composite, then the criterion is not satisfied and we then require more than one factor. We are then faced with the ambiguity that we have several batteries of tests each with its own single common factor. Which of these common factors shall we call the general one? Which is it that we should call '*g*'? Spearman's answer is that we should use a set of perceptual tests as a reference and that if we are dealing with that particular common factor, then we should call it general but that if we are dealing with one of the other common factors, then we should call it a special ability, a group factor, a sub-factor of some sort. It would be more logical to assign a letter or a name to each of these mental abilities and to treat them as related dependent abili-

ties. If we choose one of them, such as facility in dealing with perceptual relations, as an axis of reference (Spearman's '*g*'), then it should be frankly acknowledged that the choice is statistically arbitrary, for we could equally well start with verbal ability or with arithmetical ability as an axis of reference. The choice of the perceptual axis of reference might be made from purely psychological considerations, but it would not rest on statistical evidence.

The multi-dimensionality of mind must be recognized before we can make progress toward the isolation and description of separate abilities. It remains a fact, however, that since all mental tests are positively correlated, it is possible to describe the intercorrelations in terms of several factors in such a manner that one of the factors will be conspicuous in comparison with the others. But the exact definition of this factor varies from one set of tests to another. If it is this factor that Spearman implies in his theory of intelligence, then his criterion is entirely inadequate to define it, because the tetrad criterion merely tells us whether or not any given set of intercorrelations can be described in terms of one and only one common factor.

Let us start with the assumption that there may be several independent or dependent mental abilities, and let it be a question of fact for each study how many factors are needed to account for the observed intercorrelations.[2] We also make the assumption that the contributions of several independent factors are summative in the individual's performance on each one of the psychological tests. If we do not make this assumption, the solution seems well-nigh hopeless, and it is an assumption that is either explicit or implicit in all attempts to deal with this problem.

We may start our analysis of the generalized factor problem by considering the many hundreds of adjectives that are in current use for describing personalities and temperaments. We have made such a list. Even after removing the synonyms we still had several hundred adjectives. It is obvious at

[2] This point of view is represented by the work of T. L. Kelley as distinguished from the work of Spearman and his students.

the start that all these traits are not independent. For example, people who are said to be congenial are also quite likely to be called friendly, or courteous, or generous, even though we do not admit that these words are exactly synonymous. It looks as though we were dealing with a large number of dependent traits.

The traditional methods of dealing with these psychological complexities have been speculative, bibliographical, or merely literary in character. The problem has been to find a few categories, called personality types or temperaments, in terms of which a longer list of traits might be described. Psychological inquiry has not yet succeeded in arriving at a list of fundamental categories for the description of personality. We are still arguing whether extraversion and introversion are scientific entities or simply artifacts, and whether it is legitimate even to look for any personality types at all.

In the generalized factor method we have one of the possible ways in which a set of categories for the scientific study of personality and temperament may be established on experimental grounds as distinguished from literary verbosity about this subject. It is our belief that the problem can be approached in several rational and quantitative ways and that they must agree eventually before we have a satisfactory foundation for the scientific description of personality.

The problem has geometrical analogies that we shall make use of. If we have a set of n points, defined by r coordinates for each point, we may discover that they are dependent in that some of these points can be described linearly in terms of the coordinates of the rest of the points. If the adjectives were represented by these points, it is as though we were to describe most of them in terms of a limited number of adjectives. But such a solution is not unique, because if we have ten points in space of three dimensions, then it is possible, in general, to describe any seven of the points in terms of the remaining three. It is just so with the personality traits, in that a unique solution is not given without additional criteria.

But before demanding this sort of reduction it would be of great psychological interest to know *how many* temperaments

or personality types we must postulate in order to account for the differentiable traits that we use in describing people. Again the geometrical analogy is useful. If we have a table of three coordinates for each one of ten points and if that matrix has the rank 2, then we know that all ten points lie in a plane and consequently they can all be described by two coordinates in that plane instead of by three. The application of this analogy would be the description of ten traits in terms of two independent traits which would then become psychological categories or fundamental types. They would constitute the frame of reference in terms of which the other traits would be described and in terms of which interrelations could be stated.

But since we have no given frame of reference to begin with, we do not have the coordinates of the points that might represent the adjectives. We therefore let each adjective represent its own coordinate axis, so that with n traits we shall have as many axes. These coordinate axes will be oblique, since the traits are known to be at least not all independent. The projection of any one of these traits A on the oblique axis through another trait B is the cosine of their central angle, but this is also the correlation between the two traits A and B. The correlations can be ascertained by experiment, and then our problem becomes that of finding the smallest number of orthogonal coordinate axes in terms of which we can describe all of the traits whose intercorrelations are known.

The actual data that we must handle are subject to chance errors. It is therefore profitable to see how the geometrical manner of thinking about this problem is affected by the chance errors. It can be shown that each test may be thought of as a vector in space of as many dimensions as there are independent mental factors. If the test is perfect, then it is represented geometrically by a unit vector. If the test has a reliability less than unity, then the length of the vector is reduced. In fact, the length of a test vector in the common factor space is the square root of its reliability. If the test has zero reliability, then it determines nothing and this fact has its geometrical correspondence in that the test vector is then of

zero length so that it determines no direction at all in the space of mental abilities.

The obtained correlation between two tests is the scalar product of the two test vectors. If the two tests are perfect, then their scalars are both unity so that the true correlation between the two tests is the cosine of the angular separation between the two vectors.

We shall consider next two of the fundamental theorems in a generalized theory of factors.[3] Let us take a table of seven variables as an example In Fig. 1 we have shown such a table toward the right side of the diagram. We may call it the correlational matrix. In a table of this kind we show the correlation of each test with every other test. For example, the correlation between the two tests *A* and *B* is indicated in the customary cell.

In the diagonal cells of a table of intercorrelations we are accustomed to record the reliabilities, but that is incorrect in factor theory unless the tests have been so chosen that they contain no specific factors. It is necessary for us to make a distinction between that part of the variance of a test which is attributable to the common factors and that part of the variance which is unique for each test. The part which is unique for each test may again be thought of as due to two different sources, namely, the chance errors in the test and the ability which is specific for the test. The reliability of a test is that part of the total variance which is due to the common factors as well as to the specific factor. It differs from unity only by that part of the variance which is due to chance errors. We need in factor theory another term to indicate that part of the total variance which is attributable only to the common factors and which eliminates not only the variance of chance errors but also the specific variance. We have used the term *communality* to indicate that part of the total variance of each test which is attributable to the common factors. It is always less than the reliability unless a specific factor is absent,

[3] The present generalized factor theory has been described in two lithographed pamphlets by the writer. These are: 'The theory of multiple factors,' The University of Chicago Bookstore, Chicago, Ill., and 'A simplified multiple factor method,' The University of Chicago Bookstore.

in which case the communality becomes identical with the reliability. It is these communalities that should be recorded in the diagonal cells, but they are the unknowns to be discovered by the factorial analysis.

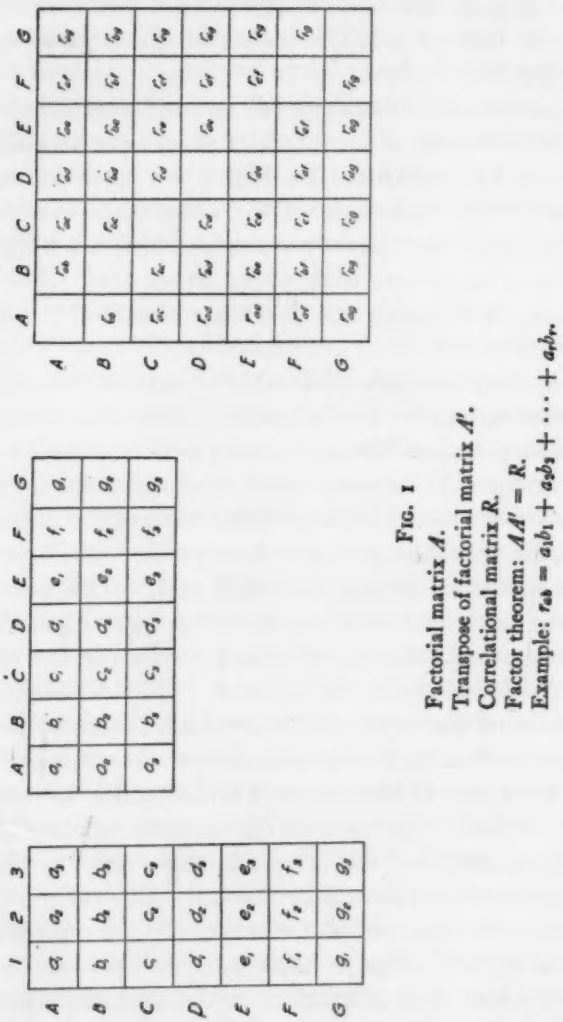

FIG. 1

Factorial matrix A.
Transpose of factorial matrix A'.
Correlational matrix R.
Factor theorem: $AA' = R$.
Example: $r_{ab} = a_1b_1 + a_2b_2 + \cdots + a_nb_n$.

At the extreme left of the diagram we have a table of factor loadings. Here the seven tests are listed vertically and there are as many columns as there are factors. In the present ex-

ample we have assumed three factors, so that we have three columns and seven rows. Let us suppose that the first factor represents verbal ability and that the second factor represents arithmetical ability. These particular abilities are not independent, but we may ignore that for the moment. Then the entry a_1 indicates the extent to which the test A calls for verbal ability and the entry a_2 indicates the extent to which it calls for arithmetical ability. Since we have assumed three factors in this diagram we have three factor loadings for each test. A table in which the factor loadings are shown for each test we have called a *factorial matrix*, while the square table containing the intercorrelations we have called a *correlational matrix*.

The experimental observations give us the correlational matrix, so that it may be regarded as known. The object of a factorial analysis is to find a factorial matrix which corresponds to the given intercorrelations. There is a rather simple theoretical relation between the given correlational matrix and the factorial matrix. This relation constitutes the fundamental theorem of the present factor theory. It is illustrated by an example under the correlation table and it can also be stated in very condensed form by matrix notation in that *the factorial matrix multiplied by its transpose reproduces the correlational matrix* within the observational errors of the given correlation coefficients.

When we want to make an analysis of a table of intercorrelations, the first thing we want to know is how many independent common factors we must postulate in order to account for the given correlation coefficients. One of our fundamental theorems states that *the smallest number of independent common factors that will account exactly for the given correlation coefficients is the rank of the correlational matrix.* Although this theorem is of fundamental significance it is not possible to apply it in its theoretical form, because of the fact that the given coefficients are subject to experimental errors and the rank is therefore in general equal to the number of tests. It is always possible to account for a table of intercorrelations by postulating as many abilities as there are tests,

but that is simply a matter of arithmetical drudgery and nothing is thereby accomplished. The only situation which is of scientific and psychological interest is that in which a table of intercorrelations can be accounted for by a relatively small number of factors compared with the number of tests.

There is no conflict between the present multiple factor methods and the tetrad difference method. When a single factor is sufficient to account for the given coefficients, then the rank of the correlational matrix must be 1, but the necessary and sufficient condition for this is that all of the second order minors shall vanish. Now if you expand the second order minors in a table of correlation coefficients you find that you are in fact writing the tetrads. Hence the tetrad difference method is a special case of the present multiple factor theorem.

It would be possible to extend the tetrad difference method by writing the expansions of the minors of higher order and in that manner to write formulae for any number of factors which correspond to the tetrads for the special case when one factor is sufficient. Such a procedure is unnecessarily clumsy. In fact, there should be no excuse for ever computing any more tetrads. Several better methods are available which give much more information with only a fraction of the labor that is required in the computation of tetrads.

We return now to the multiple factor analysis of personality. In Table I we have a list of sixty adjectives that are in common use for describing people. These adjectives together with their synonyms were given to each of 1300 raters. Each rater was asked to think of a person whom he knew well and to underline every adjective that he might use in a conversational description of that person. Since it was not necessary for the rater to reveal the name of the person he was rating it is our belief that the ratings were relatively free from the inhibitions that are usually characteristic of such a task. With 1300 such schedules we determined the tetrachoric correlation coefficient for every possible pair of traits. Since there were sixty adjectives in the list we had to determine 1770 tetrachoric coefficients. For this purpose we developed a set

Table I

List of Adjectives Used for Factor Study

persevering	religious	haughty	eccentric
crafty	impetuous	submissive	ingenious
awkward	fickle	suspicious	accommodating
self-important	domineering	courageous	tactful
determined	frank	stern	careless
friendly	pessimistic	headstrong	tidy
patient	spiteful	jealous	precise
sarcastic	quiet	generous	systematic
congenial	disagreeable	dependable	cheerful
hard-working	reserved	faithful	conscientious
stubborn	refined	reserved	grasping
capable	unnatural	solemn	satisfied
tolerant	bashful	earnest	cynical
calm	self-reliant	talented	courteous
peevish	broad-minded	frivolous	unconventional
			quick-tempered

of computing diagrams which enable one to ascertain the tetrachoric coefficients with correct sign by inspection.[4] Each coefficient can be ascertained in a couple of minutes by these computing diagrams.

The table of coefficients for the sixty personality traits was then analyzed by means of multiple factor methods[5] and we found that five factors are sufficient to account for the coefficients. We reproduce in Figure 2 the distribution of discrepancies between the original tetrachoric coefficients and the corresponding coefficients that were calculated by means of the five factors. It has a standard deviation of .069. The average standard error of thirty tetrachoric coefficients chosen at random in this table is .052.

It is of considerable psychological interest to know that the whole list of sixty adjectives can be accounted for by postulating only five independent common factors. It was of course to be expected that all of the sixty adjectives would not be independent, but we did not foresee that the list could be accounted for by as few as five factors. This fact leads us to

[4] 'Computing diagrams for the tetrachoric correlation coefficient,' The University of Chicago Bookstore.

[5] 'A simplified multiple factor method,' Univ. Chicago Bookstore, 1933.

surmise that the scientific description of personality may not be quite so hopelessly complex as it is sometimes thought to be.

Next comes the natural question as to just what these five factors are, in terms of which the intercorrelations of sixty personality traits may be described. Each of the adjectives can be thought of as a point in space of five dimensions and the five coordinates of each point represent the five factor components of each adjective.

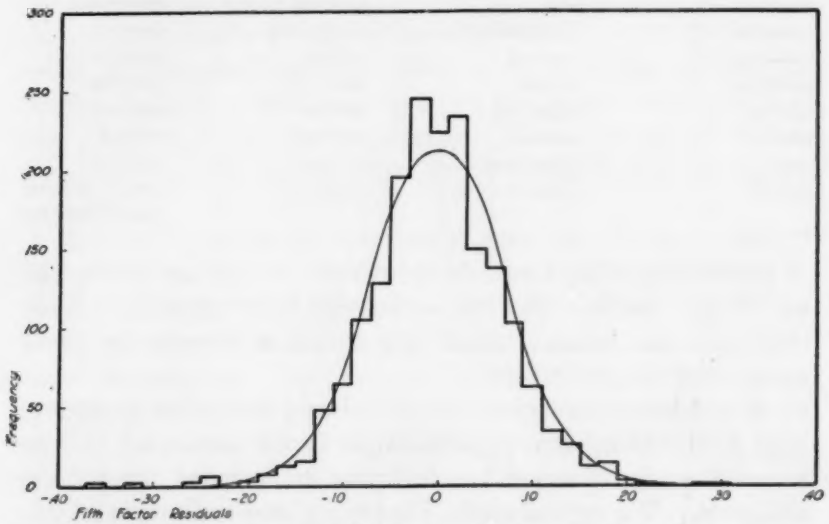

FIG. 2. Frequency distribution of fifth factor residuals in a study of sixty personality traits.

We shall consider a three-dimensional example in order to illustrate the nature of the indeterminacy that is here involved (Fig. 3). Let us suppose that three factors are sufficient to account for a list of traits. Then each trait can be thought of as a point in space of three dimensions. In fact, each trait can be represented as a point on the surface of a ball. If two traits *A* and *B* tend to coexist, the two points will be close together on the surface of the ball. If they are mutually exclusive, so that when one is present the other is always absent and vice versa, then the two traits are represented by two points that are diametrically opposite on the surface of the ball such as *A* and *D*. If the two traits are independent and

uncorrelated, such as *A* and *C*, then they will be displaced from each other in the same way as the north pole and a point on the equator, namely by ninety degrees.

Now suppose that the traits have been allocated to points on the surface of the sphere in such a manner that the correlation for each pair of traits agrees closely with the cosine of the central angle between the corresponding points on the surface of the sphere. Then we want to describe each of these traits in terms of its coordinates, but we should first have to decide

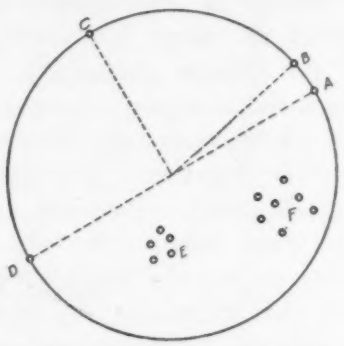

FIG. 3. Correlation *AB* is high and positive. Correlation *AC* is zero. Correlation *AD* is −1. *E* and *F* represent constellations. Location of the orthogonal axes is arbitrary.

where to locate at least two of the three coordinate axes. This is an arbitrary matter, because the internal relations between the points, that is, the intercorrelations of the traits, remain exactly the same no matter where in the sphere we locate the coordinate axes. That is, when the points have been assigned on the surface of the ball, we have not thereby located the respective coordinate axes. It is possible to determine uniquely how many independent common factors are required to account for the intercorrelations without thereby determining just what the factors are. We may therefore use any arbitrarily chosen set of orthogonal axes.

It is psychologically more illuminating to investigate constellations of the traits. In the factor analysis of the adjectives, constellations of traits reveal themselves in that the points which represent some of the traits lie close together

in a cluster on the surface of a five-dimensional sphere. One or two examples will illustrate this type of analysis with respect to the personality traits. We find, for example, that the following traits lie close together in a cluster, namely, *friendly*, *congenial*, *broad-minded*, *generous*, and *cheerful*. It would seem, therefore, that as far as the five basic factors are concerned, whatever be their nature, these several traits are very much alike as far as can be determined by the way in which we actually use these adjectives in describing people.

Another such cluster of adjectives which are used as though they signified the same fundamental trait are the adjectives *patient*, *calm*, *faithful*, and *earnest*. They cling close together in the factorial analysis. Another small group is found in the three traits *persevering*, *hard-working*, and *systematic*, which lie close together. Still another one is the cluster of traits *capable*, *frank*, *self-reliant*, and *courageous*.

It is of psychological interest to note that the largest constellation of traits consists of a list of derogatory adjectives. Such a cluster is the following: *self-important*, *sarcastic*, *haughty*, *grasping*, *cynical*, *quick-tempered*, and several other derogatory traits that lie close by. It clearly indicates that if you describe a man by some derogatory adjective you are quite likely to call him by many other bad names as well. This lack of objectivity in the description of the people we dislike is not an altogether unknown characteristic of human nature.

The schedules used in this study contained 120 adjectives, since every one of the 60 adjectives in the principal list was represented also by a synonym. This enabled us to ascertain the correlation between each pair of synonyms and we used it as an index of the consistency with which the trait was judged. Let us consider this correlation to be an estimate of the reliability of the judgments of the trait. By factor analysis we know the communality of each trait. It is that part of its total variance which it has in common with the other adjectives, namely, the five common factors. The difference between these two variances is the specificity. It shows the magnitude of the specific factor in each adjective.

TABLE II

Specificity	Adjective	Specificity	Adjective
.18	persevering	.05	broad-minded
.40	crafty	.00	haughty
.46	awkward	.07	submissive
.10	self-important	.17	suspicious
.16	determined	.27	courageous
.14	friendly	.01	stern
.02	patient	.01	headstrong
.16	sarcastic	.25	jealous
.16	congenial	.16	generous
.34	hard-working	.17	dependable
.34	stubborn	.13	faithful
.16	capable	.00	solemn
.04	tolerant	.04	earnest
.27	calm	.59	talented
.09	peevish	.39	eccentric
.58	religious	.23	ingenious
.36	impetuous	.30	accommodating
.07	fickle	.26	tactful
.19	domineering	.34	tidy
.40	frank	.22	precise
.22	pessimistic	.22	systematic
.04	spiteful	.03	cheerful
.12	quiet	.11	conscientious
.19	disagreeable	.30	grasping
.08	reserved	.68	satisfied
.27	refined	.29	cynical
.18	unnatural	.12	courteous
.48	bashful	.14	unconventional
.14	self-reliant	.41	quick-tempered

We have listed these differences for each adjective in Table II and they yield some psychologically interesting facts. We find, for example, one adjective which has a surprisingly high specificity of nearly .60, and we want to know, of course, what kind of trait it represents. We find that it is the adjective *talented*. It seems reasonable to guess that this adjective refers largely to the intellectual abilities which are not represented by this list. When this study is repeated, we shall include several adjectives of this type, so that intelligence may be investigated as a vector in relation to the personality traits.

Another item with a high specificity is the adjective *awkward*. This means that the trait *awkward* has something about it which is unique in our list of sixty and which is not

represented by the five common factors. This trait is probably ease and facility in body movement, which is certainly not represented in the rest of the sixty traits. When this study is repeated we may or we may not include several adjectives of this type, depending on whether we want to include this additional factor in a study of personality. Another adjective with high specificity is *religious* and the explanation is undoubtedly along the same lines because this is the only adjective in the list that refers to any kind of religiousness.

Studies of this sort should be repeated until every important trait is represented by several adjectives. The analysis should yield as many independent factors as may be required. When the factorial analysis is complete, the specifics should all vanish or they should be relatively small. Then the communalities and the reliabilities will have nearly the same value. The constellations to be found in such an analysis will constitute the fundamental categories in terms of which a scientific description of personality may be attained.

There is no necessary relation between the number of factors and the number of constellations. A system of tests might be found which can be accounted for by several factors even though it contains no constellations. Fortunately the constellations can be isolated in a very simple manner when the coefficients have been corrected for attenuation.

I turn next to a factor study of the insanities. I have used a very elaborate set of data which Dr. Thomas Verner Moore of Washington, D. C., collected and investigated by other factor methods. Dr. Moore worked with a list of forty-eight symptoms, thirty-seven of which are listed in Table III. He recorded the presence—absence, or a rating or test measure of each symptom for each of several hundred patients. With these records it was possible to ascertain to what extent any two symptoms tend to coexist in the same patient. For example, the extent to which the two symptoms *excited* and *destructive* tend to coexist in the same patient is indicated by the tetrachoric correlation of + .71. The records were sufficiently complete so that we could prepare a table of intercorrelations of the tetrachoric form for thirty-seven symptoms or 666 co-

TABLE III

MULTIPLE FACTOR ANALYSIS OF PSYCHOTIC SYMPTOMS

References: (1) Moore, T. V., *Amer. J. Psychiat.*, 1930, **9**, 719–738. (2) Moore, T. V., Series of Research Publications of *Ass'n for Research in Nervous and Mental Diseases*, Vol. **9**, 324–339; Baltimore, Williams and Wilkins.

Code No.	Psychotic Symptom
1.	Alcoholism of Parents
2.	Anxious
3.	Attacks, Number of Previous
6.	Delusions, Bizarre
9.	Depression
10.	Destructive
11.	Disorientation in Space
13.	Euphoria
14.	Excited
15.	Fallacies, Autistic
16.	Fallacies, Logical
17.	Giggling
18.	Hallucinations, Auditory
19.	Hallucinations, Others
23.	Homicidal
24.	Insane Relatives
25.	Insight, Absence of
26.	Irritable
27.	Memory, Total Defect
28.	Memory Ratio
29.	Mutism
30.	Negativism
31.	Neurasthenia
32.	Perception Defect
33.	Reasoning
34.	Refusal of Food
35.	Retardation
36.	Sensibilities, Loss of Finer
37.	Shut In
38.	Stereotypism of Actions
39.	Stereotypism of Any Kind
40.	Stereotypism of Attitudes
41.	Stereotypism of Words
42.	Suicidal
43.	Tantrums
44.	Tearful
45.	Voices, Speaking to

efficients. These computations were also made by the computing diagrams.

The multiple factor method was then applied to the table of 666 coefficients and we found that five factors are sufficient to account for the correlations, with residuals small enough so that they can be ignored. The communalities were then computed for each one of the symptoms and we found that about ten of the symptoms do not contain enough in common with the other symptoms to warrant their retention in a factor study. In other words, about ten of the symptoms are either so specific in character or so unreliable as to estimates that they do not yield significant correlations with the several other symptoms. This left twenty-six symptoms which are more or less related and for which the factorial clusters of symptoms could be profitably investigated.

In Table IV we have listed the psychotic symptoms which lie in each of several constellations. We find, for example,

TABLE IV

PSYCHOTIC SYMPTOMS

Cluster A, Catatonic Group	*Cluster B, Cognitive Group*
Mutism	Logical fallacies
Negativism	Total memory defect
Shut in	Perception defect
Stereotypism of action	Reasoning
Stereotypism of attitudes	Disorientation in space
Stereotypism of words	
Giggling	
Cluster C, Manic Group	*Cluster D, Hallucinatory Group*
Destructive	Bizarre delusions
Excited	Auditory hallucinations
Irritable	Other hallucinations
Tantrums	Speaking to voices

Cluster E, Depression

Anxious
Depression
Tearful
Retardation in movement

that the following symptoms are functionally closely related, namely, *mutism*, *negativism*, *being shut-in*, *stereotypism of action*, *stereotypism of attitudes*, *stereotypism of words*, and *giggling*. These seven traits are evidently related in that they tend to be found in the same patients and we recognize the list as descriptive of the catatonic group. Another constellation consists in the presence of *logical fallacies*, *defect in memory*, *defect in perception*, and *defect in reasoning*. This is a constellation of symptoms that indicates a derangement of the cognitive functions of the patient as contrasted with derangements of the affective aspects of his mentality. Another group of three symptoms that lie close together in the factorial analysis consists of the traits *destructive*, *irritable*, and *having tantrums*. A fourth cluster contains the symptoms *delusions*, auditory and other types of *hallucinations*, and *speaking to voices*. A fifth group contains the symptoms *anxious*, *depressed*, and *tearful*.

It is not likely that this analysis of the tetrachoric correlations between symptoms has given us anything like a dependable classification of the insanities, because the study can

be much improved when it is done the next time. But our results indicate that by the multiple factor methods it should be possible to arrive at a rational classification of the insanities and of personality types.

Another application of the present factor methods is an analysis to ascertain whether the vocational interests of college students can be classified in constellations and whether they can be described in terms of vocational interest types, small in number compared with the list of available occupations. These should eventually be related to the temperamental and personality traits and to the constellations of mental abilities.

Table V

Actor	Explorer	Pharmacist
Advertiser	Factory Manager	Philosopher
Architect	Farmer	Photographer
Army Officer	Florist	Physician
Art Critic	Foreign Correspondent	Physicist
Artist	Forest Ranger	Poet
Astronomer	Fruit Grower	Press Agent
Athletic Director	Geologist	Printer
Auctioneer	High School Teacher	Private Secretary
Auto Salesman	Historian	Professional Athlete
Banker	Insurance Salesman	Psychologist
Biologist	Inventor	Public Speaker
Botanist	Jeweler	Radio Announcer
Building Contractor	Journalist	Railway Conductor
Business Manager	Judge	Real Estate Salesman
Cattle Rancher	Landscape Gardener	Retail Merchant
Certified Public Accountant	Lawyer, Criminal	Sales Manager
Chemist	Lawyer, Corporation	Scientist
Civil Engineer	Librarian	Sculptor
Clergyman	Manufacturer	Secret Service Man
Club Secretary	Mathematician	Ship Officer
College Professor	Mechanical Engineer	Sociologist
Congressman	Musician	Stockbroker
Dentist	Newspaper Reporter	Surgeon
Diplomatic Service	Novelist	Tax Expert
Economist	Office Manager	Vocational Counselor
Electrical Engineer	Orchestra Conductor	

We asked three thousand students in four universities to indicate their likes and dislikes on a list of eighty of the better known occupations (Table V) which are available for

college students. The tetrachoric correlations indicate the extent to which those who are interested in engineering, for example, can also be expected to have some interest in physics, or in chemistry. Those who are interested in engineering tend to dislike law and journalism and salesmanship, so that these correlations are negative. Our first question will be to ascertain the number of factors that are necessary to account for the observed intercorrelations. Previous inquiry indicates that the number of types may not exceed six or eight.

The scoring of the individual schedules might be reduced to a number of scores equal to the number of factors. These scores would be the coordinates of a point which represent the subject in a space of as many dimensions as there are factors required by the intercorrelations. The occupational likes and dislikes of the subject might be estimated by the coordinates of the point which represent his own interests. In this manner it may be possible to make a limited list of occupations that are typical of the interests of each student.

The question might be raised whether a student should be advised to enter that occupation for which his interests are typical. It might well be argued that he has a better chance of success if he enters a profession for which his interests are unusual. But that question refers, of course, to educational and vocational guidance while our present problem concerns the methodology of isolating types or constellations of traits. If we should find that vocational interests group themselves into a relatively small number of types it would have psychological interest in relation to the personality traits and mental abilities of the same subjects. It would be another question to decide how such data could or should be used in guidance.

An application of the factor methods has recently been made by Mrs. Thurstone. Her problem was to ascertain whether radicalism is a common factor in people's attitudes on various disputed social issues. She gave eleven attitude scales to about 380 students in several universities. Records of the intelligence examination of the American Council on Education were available for the majority of these students, so that

the study includes twelve variables. The factorial analysis reveals a conspicuous common factor that we recognize as radicalism. The second factor residuals were comparable with the standard errors of the given coefficients. The standard error of the mean correlation coefficient was .047 while the standard error of the second factor residuals was. 056.

Since the two factors were sufficient to account for the intercorrelations, we have plotted in Figure 4 the factor loadings as the two coordinates for each of the twelve variables. Several

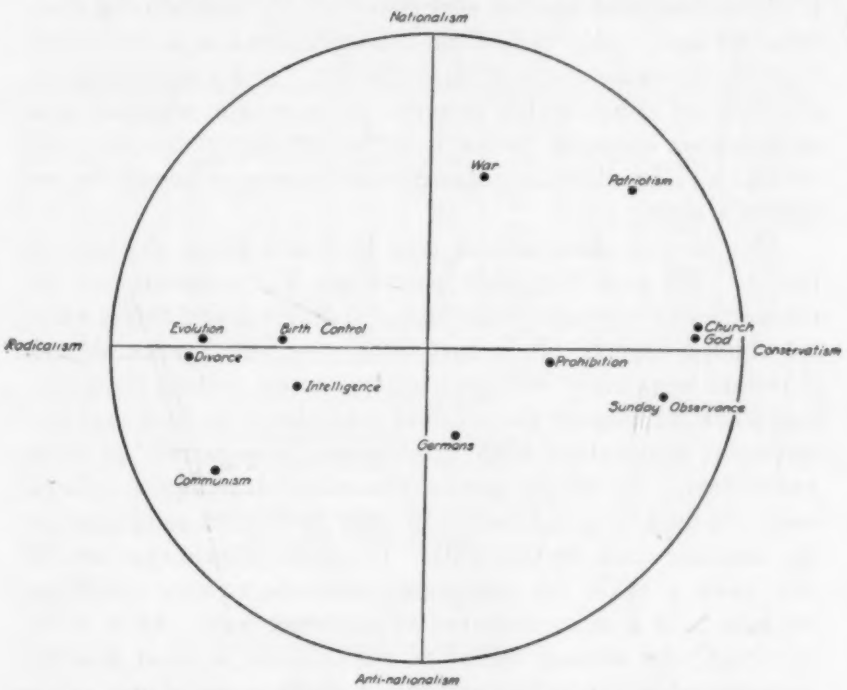

FIG. 4. Factor study of radicalism with attitude scales by Thelma Gwinn Thurstone.

psychological interpretations can be made from this diagram. The variables which are heavily loaded with radicalism are attitudes favorable to evolutionary doctrines, favorable to birth control, favorable to easy divorce, favorable to communism, and it is of interest to note that intelligence is positively correlated with these radical or liberal attitudes. In the opposite direction we find conservatism in attitudes favorable to the church,

favorable to prohibition, the observance of Sunday, and belief in a personal God. Inspection of the original coefficients as well as the factorial analysis shows the more intelligent college students tend to be radical or liberal on social questions and that they tend to be atheistic or agnostic on religious matters.

In naming the common factor which is most prominent in these attitude scales there may be some question as to whether it should be called conservatism or religion. The most pious people are likely to be against evolutionary doctrines, against birth control, and against easy divorce. It may also be that what we have called radicalism-conservatism is a factor which is nearly the same as the religious factor. It is a psychological question of considerable interest to ascertain whether this conspicuous common factor is to be attributed to temperamental and intellectual differences in people or merely to religious training.

One or two observations may be made about the second factor. We note first that patriotism and communism are diametrically opposite in the factorial diagram and this is what we should expect. It is surprising that intelligence should correlate negatively with patriotism among college students; but when we inspect the original coefficients we find that the strongest correlation with intelligence is negative .44 with patriotism. In other words the most intelligent college students tend to be lukewarm in their patriotism as judged by the attitude scale for this trait. It is unfortunate that we did not have a scale for measuring attitude toward pacifism, because it is a more disputed object than war. As is to be expected, the second factor of nationalism is most heavily represented in patriotism and in the glorification of war, while the opposite attitude, namely, international-mindedness, is represented by the attitudes of those Americans who are friendly toward communism and toward Germany. These examples will suffice to illustrate the possibilities of factorial analysis in dealing with the affective and temperamental attributes of people.

Until recently practically all of the studies that have been made by factor methods have been confined to the cognitive

traits and especially to the mental abilities that are represented by psychological tests. It is on this type of material that Professor Spearman and his students have worked extensively with the tetrad difference method. I shall describe the analysis of a set of nine psychological tests which have been investigated by Mr. W. P. Alexander of the University of Glasgow and I reproduce here with his per-

TABLE VI

CORRELATIONS OF NINE TESTS USED BY W. P. ALEXANDER

Westfield State Farm—71 Cases

Correlation Matrix

Test	1	2	3	4	5	6	7	8	9
1.....	—	451	373	337	717	750	768	786	612
2.....	451	—	428	470	277	393	407	557	616
3.....	373	428	—	362	255	402	415	334	415
4.....	337	470	362	—	189	304	257	203	528
5.....	717	277	255	189	—	695	752	800	335
6.....	750	393	402	304	695	—	834	850	478
7.....	768	407	415	257	752	834	—	917	433
8.....	786	557	334	203	800	850	917	—	450
9.....	612	616	415	528	335	478	433	450	—

Description of Tests

1. Stanford-Binet.
2. Pintner-Paterson Scale.
3. Healy Puzzle II—Picture Completion.
4. Porteous Maze Test.
5. Thorndike Reading Test.
6. Otis Group Test.
7. Otis Self-Administering Test.
8. Terman Group Test.
9. Alexander Performance Scale.

mission a section of his unpublished data (Table VI). The nine tests are all well known except the new performance test which he has recently devised. When we apply the multiple factor methods to this correlational matrix we find that most of the variance of each test can be accounted for by postulating only two factors. These we have plotted in Fig. 5.

In this diagram the abscissæ represent the first factor that was extracted and the ordinates represent the second factor. As is to be expected the centroid of the whole system is on the

x-axis since this is implied in the approximation procedure that was used for this problem. Even in a first glance at this diagram we are struck by the fact that the nine tests divide themselves into two groups or clusters. We turn immediately to the tests to see what psychological abilities may be found in the tests that constitute each of these two constellations. We then find that all of the verbal tests fall in one of these constellations and that all of the performance tests fall in the other

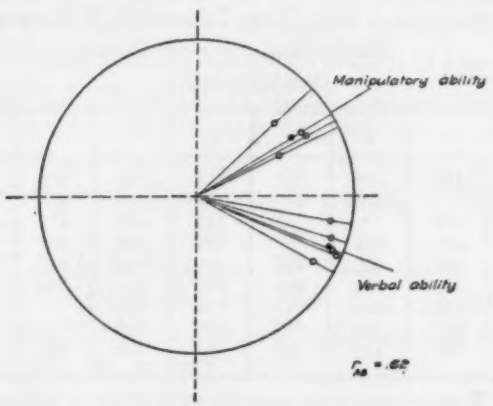

FIG. 5. Two factor analysis of nine tests used by W. P. Alexander.

constellation. This suggests a rational method of establishing the psychological categories that we should call *mental abilities*. There seems to be little doubt in naming one of these constellations *verbal ability* and we shall name the other one tentatively as *manipulation*, since that is common to all of the performance tests. The verbal tests that cling together in this analysis are the Stanford-Binet, the Thorndike reading test, the Otis group test, the Otis self-administering test, and the Terman group test. The performance tests that group themselves apart from the verbal tests are the Pintner-Paterson scale, the Healy Puzzle II (picture completion), the Porteus maze test, and Alexander's new performance test.

It is to be noted that these two constellations are entirely independent of the location of the two orthogonal axes through this system of nine tests. A characteristic of the multiple factor problem is that the location of the axes is arbitrary and

that hence the factorial components are to that extent arbitrary and without fundamental psychological significance. This limitation is entirely obviated if we center our interest on the constellations of mental traits that are revealed in the factorial analysis. The relations between the constellations are invariant under rotation of the orthogonal axes, and hence we have here something more or less permanent in terms of which we may define psychological categories and mental abilities.

As illustrative of this method we have found the centroid of each of the two constellations in Alexander's nine tests. These centroids are indicated by the small black circles. If we had a fairly large number of tests in each constellation we could attach some confidence to the centroid of the constellation as a definition of a mental ability. I am here using the term *mental ability* to identify a constellation of tests which lie in the same cluster in the factorial analysis and which correlate nearly unity when they are corrected for attenuation.

In Alexander's data we find that the two constellations are not independent. This is seen on the diagram by the fact that the central angle between the two constellations is not a right angle. Since all mental abilities are positively correlated, we should expect that all of the constellations of mental abilities will be positively correlated also, and that is the case in the present data. Now if we are to use these constellations of traits as the categories for psychological description and if we are to define mental abilities and personality types in terms of constellations, then it is immediately apparent that our categories will not all be independent. This indicates that we must look eventually to still more fundamental categories in terms of which to describe the mental abilities. In the meantime it will be useful to describe them in terms of constellations of known degrees of dependence and we shall then be using a system of oblique coordinates instead of the orthogonal coordinates of conventional mathematics. Ultimately we shall want to find that particular location of the orthogonal axes which corresponds to independent genetic elements.

I shall now describe briefly the results of a multiple factor

analysis of a set of twenty tests that were recently used by Dr. William Brown in support of the Spearman single-factor hypothesis of intelligence. We can sympathize with Dr. Stephenson who computed all of the fifteen thousand tetrad differences for this table and we shall not even venture to guess how long it must have taken to make the computations.

The multiple factor method in its simplest approximation form was applied to this set of data, and we find that two

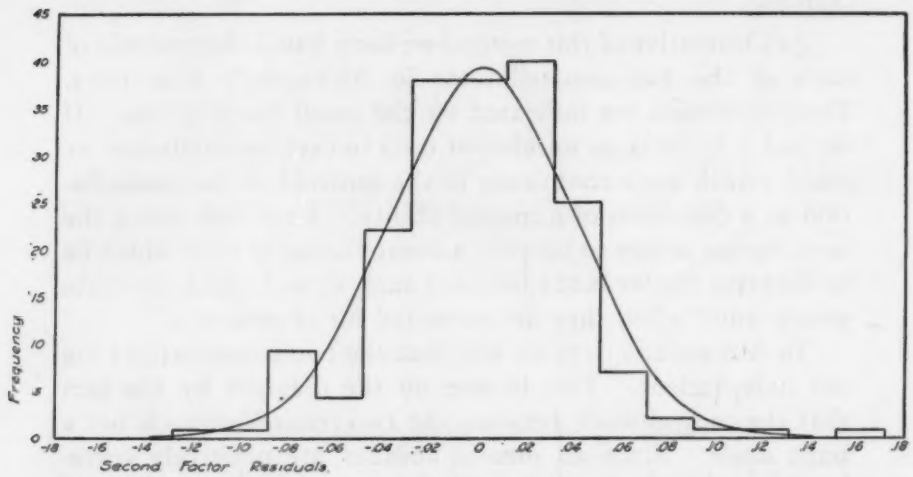

FIG. 6. Frequency distribution of second factor residuals for 20 tests used by Dr. William Brown. 'A test of the theory of two factors,' *Brit. J. Psychol.*, April, 1933, Approximate solution by multiple factor methods.

factors are sufficient to account for the given coefficients. The distribution of second factor residuals is shown in Fig. 6. The standard deviation of the distribution of residuals is .039. In Fig. 7 we have plotted the two-factor coordinates for these twenty tests and we then see that they fall into two constellations. Reference to the tests shows that all of the verbal tests are in one group and that all of the perceptual tests are found in the other group. We might regard these two groups as representing two mental abilities, one of which we should call verbal ability, while the other one might be called visual form perception. The correlation between pure measures of these two abilities is the cosine of the angular separation between the two centroids.

Brown's interpretation of these relations which he has investigated with the tetrad difference method is that one of these two clusters represents Spearman's '*g*' and that whatever is left over is attributable to verbal ability. It seems to us that it would be just as logical to call the verbal cluster the general one and to attribute the residuals to a special perceptual factor. In that case the verbal constellation would be called the general '*g*.' This reasoning might be extended to each one of the many constellations of abilities which are

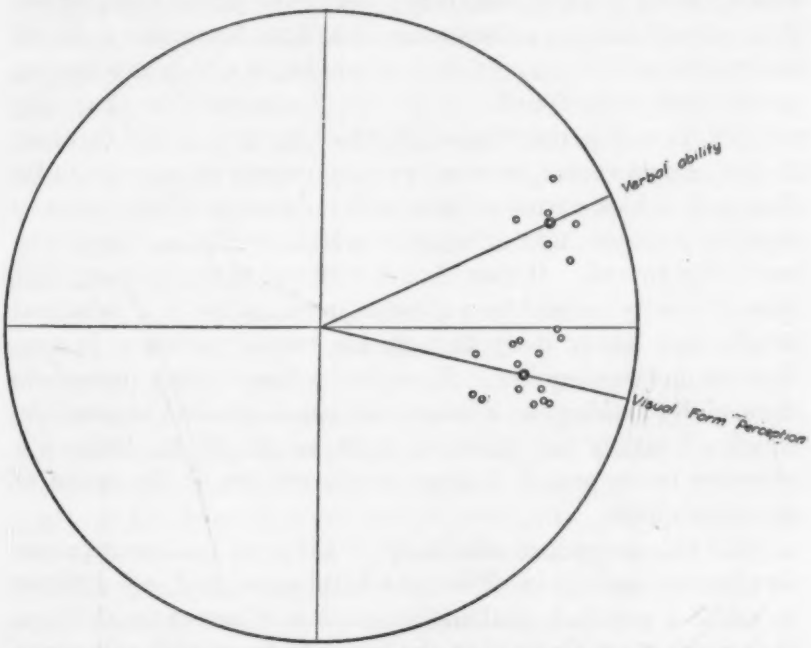

FIG. 7. Two-factor analysis of the twenty tests used by Dr. William Brown in 'A test of the theory of two factors,' *Brit. J. Psychol.*, April, 1933.

represented by groups of psychological tests. But the evidence is as yet entirely insufficient to demonstrate that any one of the constellations of mental abilities is a principal intellective factor.

Since all mental tests are positively correlated, it follows that the tests must lie in a limited quadrant or octant of the geometrical representation of the mental abilities. Instead of

adopting one of the constellations as a reference it might be more logical to find a point in the octant of the mental abilities such that the most diversified series of tests all correlate positively with the imaginary test at this central point. It is not inconceivable that a point might be located by this means with some degree of certainty. It would then represent the central intellective factor and those tests would be the best tests of intelligence which correlate highest with the imaginary pure test, which is represented by this central vector. It would not be the centroid of any particular set of tests unless these should happen to be evenly distributed over the space of the mental abilities and that is a condition which can not be guaranteed beforehand. It is not inconceivable that the constellation of perceptual relations of Spearman will lie close to this central vector, because we may expect those tests to lie close to it which correlate best with judgments of intelligence, and that is the practical criterion by which intelligence tests have been constructed. If that should turn out to be the case, then Spearman's '*g*' would be a close representation of a principal intellective factor even though his tetrad criterion is ambiguous and inadequate. If we follow Spearman's procedure in carefully picking out a list of tests, as diversified as possible, which all satisfy his criterion, then we should be defining a common factor which is close to the center of the space of mental abilities.

But this procedure necessarily leads to at least some negative factor loadings in all but the first factor, and it is difficult to make a psychological interpretation of negative abilities. It is much more likely that the opposite procedure will give a solution that makes sense for psychology and for genetics. This solution would be to find a set of orthogonal axes through the fringe of the space of mental abilities rather than through the middle of it. The geometrical representation of the solution will probably be as follows. The mental abilities can be represented as points within a cone. The axis of the cone will represent a fictitious central intellective factor. The fundamental abilities which have genetic meaning will be represented by a set of orthogonal elements of the cone in space of

as many dimensions as there are genetic factors. All mental tests will then be described in terms of positive orthogonal coordinates, corresponding to the independent genetic factors. Negative loadings will disappear. For rough and practical descriptive purposes the axis of the cone of mental abilities may be used as an axis of reference which is central in the space of mental abilities. It will be a fictitious central intellective factor but it will probably have no fundamental psychological or genetic meaning.

In conclusion I want to suggest the course of investigation which is likely to lead to a scientific description and understanding of mental abilities and personality traits and their aberrations. Our first task is to establish the identity of the several mental abilities which reveal themselves as distinct constellations in factorial analysis. Among these abilities it is quite likely that we shall find verbal ability, perceptual relations, and arithmetical ability to be distinct, though positively correlated.

In these studies it is probably best not to pivot on any single constellation as fundamental to all of the rest. It is better to use an analysis which allows as many factors to appear as are necessary to account for each new set of tests and to name the constellations when they appear. These categories should be frankly regarded as temporary and subject to redefinition in successive experiments. Eventually we should be able to work with a rather limited number of mental abilities and trait constellations. These categories will be either more or less conventionalized, or else they will be so consistent that some physical or even genetic significance may be attached to them.

In extracting each constellation it is essential to make sure that the specific variance in each variable has vanished or that it is small enough so that it can be ignored. If one of the variables retains a considerable specific variance after the common factors have been extracted, it is necessary to repeat the experiments with additional tests which are similar to the one which has shown a specific variance. Only when the reliability and the communality are nearly equal can we be sure

that the common factors account for the total variance in each test. In order to favor this outcome it is best to assemble the test batteries in such a way that there are several similar tests of each kind that are to be investigated. This is in a sense the opposite of the precautions which have been current in factor studies where the experimenters have been careful to avoid similar tests simply because they disturb the tetrad criterion. Instead of concealing the specific variance with the chance errors, as is done in tetrad analysis, it is more illuminating to investigate the nature of the specific variance of each test by including several similar tests of each kind in the test battery.

When the mental abilities have been defined in terms of a large number of elementary tests for each ability, it will be of considerable interest to ascertain experimentally the extent to which training of one ability transfers to another ability and to relate such transfer effects to the known correlations between the abilities.

It is my conviction that the isolation of the mental abilities will turn out to be essentially a problem in genetics. It will be profitable to ascertain what part of the variance of each ability can be attributed to the parents. It is not unlikely that this type of genetic research will constitute one of the means for identifying the independent elements in the mental abilities.

We hope that the development of factorial methods of analysis will give us the tools by which to reduce the complexities of social and psychological phenomena to a limited number of elements. These methods should be useful not only for developing the theory of mental abilities and temperamental traits but also in meeting the practical demands of educational and psychological guidance.

[MS. received October 27, 1933]

THE CONCEPT OF THE HABIT-FAMILY HIERARCHY AND MAZE LEARNING:[1] PART I

BY CLARK L. HULL

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I. The Principles of Divergent and of Convergent Associative Tendencies

Our conception of habit is necessarily expanded with the increase in our comprehension of its phenomena. Instead of presenting a single unvarying and indistinguishable sameness, as is too often assumed, habits, even to our present meager knowledge, present a remarkably varied series of patterns. In this multiplicity it is possible, from the point of view of the present paper, to distinguish two major types. One type is characterized by a fan-like series of divergent excitatory tendencies radiating from a single stimulus, each leading to a distinct reaction. This type will be called the *divergent* mechanism. The other is characterized by the convergence of a number of excitatory tendencies from separate

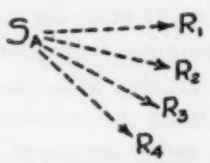

Fig. 1. The divergent excitatory mechanism. Ordinarily the different tendencies will be of widely varied strength.

stimuli, all upon a single response. The second type will consequently be called the *convergent* mechanism. The divergent mechanism is represented diagrammatically in Fig. 1,

[1] The substance of the major portion of the material contained in this paper has been used as a bound manuscript in certain of the writer's classes since June, 1931. During that time the author has received many valuable criticisms and suggestions from his students. Those of Dr. Kenneth W. Spence and of Mr. Neal E. Miller have been particularly valuable.

the convergent mechanism, in Fig. 2 (13). In these diagrams and those to follow, the *S*'s represent stimuli and the *R*'s represent responses. Arrows with broken lines indicate acquired tendencies to action; arrows with solid lines indicate unlearned tendencies to action.

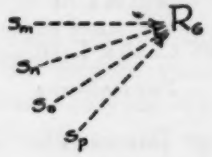

FIG. 2. The convergent excitatory mechanism.

As might readily be anticipated from the sharpness of the contrast between these two stimulus-response mechanisms, the functions performed by each in mediating adaptive behavior are characteristically distinct. Compound habit mechanisms have not been sufficiently studied from the present point of view to warrant any ultimate or comprehensive statement regarding the functional dynamics of either of the types. It is clear, however, that the first or divergent mechanism, in case the several radiating excitatory tendencies within it are antagonistic (incompatible with one another in the sense that they cannot be executed by the organism simultaneously) will give rise to a competition among themselves. This identifies the mechanism at once as the basic one responsible for the variability of response in trial-and-error learning (13). When originating in the drive stimulus (S_D), this mechanism also appears to bring about the intra-serial competition which is responsible for the serial segment elimination or 'short-circuiting' characteristic of pure-stimulus or symbolic act sequences (14). Much the same mechanism seems also to be responsible for anticipatory intrusions (22, 23), particularly of the fractional component of the anticipatory goal reaction which has been supposed to bring about the phenomenon of ideomotor action and its tremendously important functional correlate, guiding or directive ideas (15). No doubt other and equally important functions performed by this mechanism are yet to be discovered.

The second or convergent type of compound excitatory tendency clearly presents as a primary characteristic a response equivalence of the various stimuli involved; S_m, S_n, S_o, etc., are all functionally equivalent in that all alike evoke R_G. The functional dynamics of this mechanism have been studied even less than have those of the divergent mechanism, but it is believed that they have far-reaching implications for the explanation of the more subtle forms of mammalian adjustment. In particular there is reason to believe that functional equivalence of stimuli plays an important rôle in bringing it about that habits established under certain stimulus conditions will function with little or no delay in new situations having nothing whatever as *objective* stimuli in common with the conditions under which the habit was originally formed.

This capacity to react appropriately to wholly new stimulus situations is of enormous biological importance because it not only saves the organism the labor of learning by trial and error to react to each new situation as it arises, but, in cases of danger where prompt and appropriate defense reactions are needed, it may make the difference between continued life and sudden death. In many such cases practice of the ordinary trial-and-error variety would be maladaptive because even one error might be fatal. This particular mechanism has an added theoretical interest because certain writers (**19**, 170) [2] seem to have supposed that the types of behavior mediated by it are quite impossible of explanation by any principle of association or habit action.

II. Associative Convergence a Mechanism of Automatic Habit Transfer

The power of the convergent mechanism to mediate transfer of reaction from one situation to a second which, *objectively* considered, may be totally different, is not difficult to show. Suppose that after the formation of the excitatory tendencies shown in Fig. 2, the combination $S_m \rightarrow R_G$ has been extended by conditioning or otherwise into a sequence

[2] Throughout this paper, numbers in bold-face type refer to items in the list of references; the associated numbers refer to pages of publications cited.

(Fig. 3) which eventuates in the terminal defense reaction R_X. Now suppose, after this has taken place, that the organism is stimulated by S_p. The sequence which would be expected to follow on the principle of ordinary habit activity is shown diagrammatically in Fig. 4. By Fig. 2, S_p must give rise to

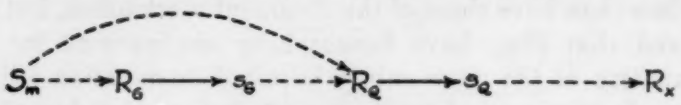

FIG. 3. A chain reaction formed by extending one of the convergent excitatory tendencies shown in Fig. 2.

R_G and this, in turn, to s_G. But since s_G has been conditioned to R_Q, the chain of reactions leading to R_Q will be initiated much as in Fig. 3, with the result that S_p, even though never associated with it, will bring about the terminal reaction R_X. There will be the difference, however, that R_Q will not have the support of the remote excitatory tendency from S_m (14, 519) which it had in the original sequence (Fig. 3).[3]

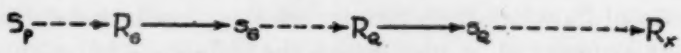

FIG. 4. Diagram illustrating the possibility of a transfer of reaction originating from one stimulus (S_m) to another (S_p) which is assumed, objectively, to be totally different.

Thus we observe the superficially impossible is really quite easily conceivable without the assumption of any principles beyond the commonplace ones of conditioning or habituation. From a common-sense point of view, the explanation of the paradox lies in the fact that while, objectively considered, S_m and S_p are entirely distinct, they have, possibly by chance association, acquired identical excitatory potentialities in the body of a particular organism. This identity of excitatory capacity brings about substantially the same outcome as if they possessed an objective identity (12).[4]

[3] In case the sequence of Fig. 3 were highly patterned (15, p. 500, note), e.g., by a large amount of over-training, the reaction shown in Fig. 4 would probably become exceedingly weak and might not take place at all, because the remote bond $S_m \rightarrow R_Q$ would be lacking in the stimulus combination shown in Fig. 4.

[4] As a result of this difference in the basic mechanism involved, it is to be expected that transfer on the basis of objective identity in the stimulus situation should be largely common to the members of a given species, whereas transfer based on acquired

The habituation transfer mechanism presented above abstractly may be clarified and emphasized by means of an experimental example. Such a case on the conditioned-reaction level seems to be furnished by a recent investigation reported by Shipley (**28**). Eleven human subjects were first stimulated by a faint flash of light, followed by the tap of a padded hammer against the lower eyelid. This has been shown by experiment normally to give rise to a conditioned tendency for the flash to evoke the wink. Next the subject was stimulated repeatedly by an induction shock on the fingers of one hand which, in addition to the sharp retraction of the hand from the electrodes delivering the shock, normally also gives rise to a wink. Here, then, we have two stimuli independently able to evoke the same reaction, which constitutes the convergent mechanism. This is shown diagram-

FIG. 5. Shipley's convergent conditioning situation. Here as elsewhere in the present study, the solid line represents what is presumably an unlearned tendency, and the broken line, an acquired tendency.

matically in Fig. 5, exactly paralleling the relationship shown in Fig. 2.

After the training with the shocks, the flash was given alone and, in a small percentage of the subjects, this now evoked a finger retraction despite the fact that the *flash had never previously been associated either with the shock or the finger retraction.* A control experiment involving the same number of shocks similarly timed, etc., gave no finger retractions, which would seem to make it reasonably certain that the results in the case cited above were not due to mere irradiation.

The observed results of Shipley's experiment may easily be deduced from the principles of ordinary conditioning, just as was the case of transfer abstractly considered above. Since

identical excitatory potentialities should be very unevenly distributed within a population. Such generality as the latter would show, would be based on the similarity of environmental circumstances. In some cases even this might produce considerable uniformity in transfer capacity.

the shock evokes both the wink and the retraction, the proprioceptive stimuli from each should become conditioned to the other reaction either by a simultaneous, or at least by a backward (31), process, as shown in Fig. 6. Accordingly, when later the organism is stimulated by the flash, this gives

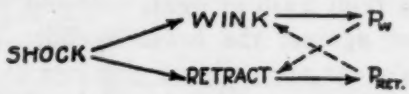

FIG. 6. Diagram showing how mutual conditioned reactions may be set up among the several part-reactions to a single stimulus.

rise to the retraction by means of the stimulus-response sequence shown in Fig. 7. Naturally the tendency would be very weak, in part because of the specific weakness of simultaneous or backward conditioned tendencies (38), and doubt-

FIG. 7. Diagram showing the hypothetical manner in which the flash was able to evoke the finger retraction with which it had never been associated (Shipley's 'Group *C*').

less also in part because the transfer involved the breaking up of a stimulus pattern.

III. Associative Divergence and Convergence the Basis of the Habit-Family Hierarchy

With this introduction to the mechanisms of divergent and convergent excitatory tendencies, we may now turn to our main concern, which is a higher synthesis obtained by combining the two into a single functional unit. The synthesis in question is represented diagrammatically in Fig. 8. It will be observed that this figure begins with the divergent mechanism of Fig. 1, and ends with the convergent mechanism of Fig. 2. Extending between the two and connecting the S_A with the R_G are a number of distinct behavior sequences which are supposed to differ greatly from one another both as to the qualitative nature of their activity and as to length of sequence, the first sequence (initiated by s_1) being shortest, the second and third progressively longer, and the fourth longest of all.

It is to be expected that the adaptive potentialities of the mechanism obtained by combining the two dynamic tendencies discussed above should differ in certain respects from those manifested by either alone. Whereas the one mechanism by itself mediates alternative reactions, and the other brings about transfer through equivalence of reaction potentiality, there emerges from the combination (Fig. 8) the sig-

FIG. 8. The habit family hierarchy which consists of combining the divergent and the convergent excitatory mechanisms shown in Figs. 1 and 2. The length of the sequences represents inversely the excitatory strength of each at point S_A. It is probable that this mechanism is related to Tolman's 'means-end-field' (33, 177).

nificant novel phenomenon of the *habit family*. A family of habits may be defined in general as a group of two or more habit sequences, all of which may be initiated by a particular stimulus and terminated by a particular reaction.[5] Despite the 'emergent' nature of the habit-family mechanism, it nevertheless retains clear functional evidences of its origin. The individual habit sequences of a particular family are still alternative in that but a single member can be active at once. At the same time they are equivalent in that all are alike in bringing about substantially the same final reaction or adjustment to the problem situation presented by S_A.

Perhaps the simplest and most obvious examples of habit families are found in alternative locomotor sequences through space. If, while following a footpath we encounter a pool of water, the problem may be solved by detouring in either the right-hand or the left-hand direction. The respective behavior sequences are alternative in that we cannot go in both

[5] The term 'families' is here used in much the same sense that geometers use the term to designate a series of curves, such as parabolas, which originate at the same point but thereafter follow different courses, all being generated by a single formula but each having a different value for one of the parameters.

directions at the same time. They are equivalent in that each without distinction brings us again upon our path.

The alternative-pathway maze, represented conventionally in Fig. 9, gives rise to what, from the point of view of simplicity, is a limiting case of the habit-family hierarchy. As in the diagram, it is usual for such mazes to have one path somewhat shorter than the other. On the basis of trustworthy experiment (**2**), it is known that the shorter of the two paths, presumably in conformity with the principle of the goal

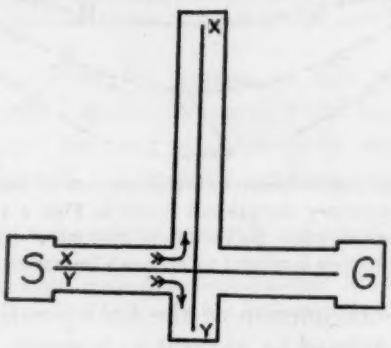

FIG. 9. Example of a simple habit family hierarchy which has been investigated experimentally. Path *Y* is the preferred member of the hierarchy.

gradient (**16**), will come to be preferred by animals which have become familiar with both. The fact that both of the action sequences begin with the same stimulus (*S*) and terminate in the same reaction (*G*) constitutes them a habit family; the fact that one sequence is preferred above the other constitutes them a hierarchy. By increasing the number of pathways and keeping the pathways all of a different length, there obviously may be generated in an animal fairly large habit-family hierarchies.

It should be especially noted, however, that the principle of habit-family hierarchy is not limited to the field of spatial orientation here emphasized. It is operative in all situations wherever there is more than one distinct action sequence which will lead to the attainment of a particular goal or subgoal. It is believed, for example, that the habit-family hierarchy constitutes the dominant physical mechanism which mediates

such tests of truth and error as organisms employ—that it provides the basis for a purely physical theory of knowledge.[6]

IV. The Habit-Family Hierarchy and the Automatic Transfer of Practice Effects

The deductive explanations of maze phenomena which will be attempted in the following pages rest, for the most part, upon the following two related hypotheses:

1. *That in the albino rat, habit-family hierarchies based primarily on locomotion are set up very early in life, presumably as the result of locomotion in free space, with the result that the animal at the beginning of a maze experiment is already in possession of a vast repertoire of equivalent but fairly distinct locomotor habits, any one of which, in free space, would mediate a transition of his body from the starting point to the goal.*

2. *That when one member of a habit-family hierarchy has attained a goal in an objectively novel situation, the learning thus acquired is transferred without specific practice to the remaining members of the hierarchy.*

A crucial experimental test of the incidental assumption in the first hypothesis (that locomotor habit-family hierarchies are acquired by early running in free space) is difficult. Some general indications as to its validity might be obtained, however, by determining whether *very* young rats, *e.g.*, animals under ten or (possibly) twenty days of age, would manifest less strongly the phenomena to be expected on the assumption of the truth of the hypothesis than would older animals. Another test, though again not a crucial one, might be made by determining whether rats raised in total darkness, suspended in a hammock through which their legs would protrude so as to touch a miniature treadmill, say (thus allowing free movement and a certain amount of exercise but no genuine transit in space), would show as strong evidences of the existence of a habit-family hierarchy as would animals of the same age reared under ordinary conditions. The hypothesis leads to the expectation that they would not. By similar reasoning, animals reared in

[6] This matter is reserved for elaboration in a subsequent paper.

very small cages would show less evidences of orientation than animals raised in a cage as large as an ordinary room.

The validity of the second hypothesis must be judged in the main by the extent to which the deductions based primarily upon it agree with the facts of mammalian behavior as the latter shall finally come to be known. It is exactly this type of evidence which supports such belief as we have in molecules, atoms, and electrons.

It may be stated at once, however, that this hypothesis is definitely not put forward as an ultimate principle. On the contrary, it is confidently expected that in case it turns out to be sound, it will be found to be deducible from certain still more general principles.[7] As an illustration of the general course which this deduction (or perhaps more properly, *reduction*) is likely to take, the following sketch is offered. The principles are fairly simple and the most of them are reasonably well authenticated.

From the nature of the habit family, it follows that in it all behavior sequences terminate at the same point, or in the same final act. This will often be consummatory in nature. Now there is considerable evidence that terminal reactions have a strong tendency to come forward in their entirety, as shown by abortive intrusions into behavioral sequences, which sequences were originally antecedent. Not only this, but there is evidence (**29**; **30**) that reactions some distance from the goal also tend to come forward in the behavioral sequence, though these latter reactions appear to possess less strength in this respect than do goal reactions or reactions closely antecedent to the goal reactions. There is reason to believe, however, that in case the goal reactions or pre-goal reactions as a whole are in physiological conflict with the sequence properly leading to them, some *portion* of the goal reaction which chances not to be in such conflict will be temporarily detached from the goal aggregate and will move forward in the sequence independently and rather freely (**15**).

As yet this subject has been little investigated experimentally, so that the extent and variety of such fractional

[7] This statement also holds for the principle of the goal gradient (**16**).

anticipatory reactions is very imperfectly known. Fortunately, salivation, a well-known type of anticipatory reaction, has been extensively studied. It is of significance for the understanding of the probable mechanism mediating transfer in the habit-family hierarchy that the amount and quality of anticipatory salivation is known to vary widely according to the adjustment situation (**25**, 184). Much less is known experimentally about fractional anticipatory sex behavior, though clearly this species of behavior manifests not only complex secretional activities but extensive and profound circulatory changes as well. Tumescence may be mentioned as a characteristic fractional anticipatory sex reaction. In addition to these rather gross and obvious forms of fractional anticipatory behavior, it is probable that rich variety exists, of which we as yet know little or nothing. It is conceivable, for example, that the mere visual stimulation by a light pattern may evoke hidden but characteristic reactions in the organism which could play the anticipatory rôle. At all events, it is not purely gratuitous to assume that both goal reactions and pre-goal reactions may possess characteristic fractional components which will be drawn forward to the beginning of all action sequences originally leading to them, and which, after each family has attained a moderate degree of integration, will be present everywhere throughout all members of a given family.[8]

It thus seems probable that the fractional anticipatory goal reaction (r_G, Fig. 10) is the major mechanism which brings about the integration of the habit-family hierarchy. It is this mechanism which appears to be mainly responsible for the important and characteristic phenomenon of the transfer of practice effects from a less preferred member of a habit hierarchy to a more favored one. There is also reason to believe that under certain circumstances the fractional anticipatory goal reaction will be powerfully supported in this action by the drive stimulus (S_D, Figs. 10 and 11) in

[8] A somewhat detailed discussion of one mechanism by which goal and near-goal reactions may be displaced forward in a behavior sequence may be found in reference 14. A second mechanism has been proposed by Lepley (21).

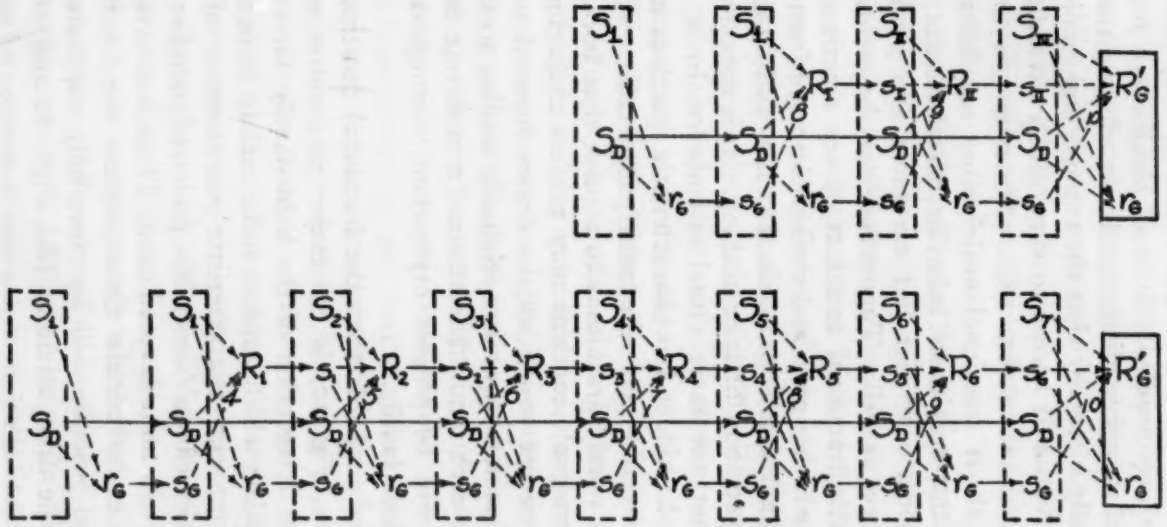

FIG. 10. Diagrammatic representation of the more important excitatory tendencies involved in two behavior sequences belonging to a habit-family hierarchy. Numbers corresponding to a conventional rectilinear goal gradient are inserted on the various lines representing excitatory tendencies from s_G to the several R's. It is to be noted that s_G leads to R_I with a strength of 8 units and to R_1 with a strength of only 4 units. The repetition of S_1 at the outset indicates a hesitation of the animal until S_1 has had time to arouse r_G.

case the latter is the same in the new situation as in the old. Even in cases where the drive may be superficially distinct, there is some reason to believe that there may be an inner core of identity which, if present, would doubtless aid considerably in mediating a transfer. While the inadequacy of our knowledge about these matters is such that we must be very tentative at present as to the precise details of the processes involved, it may be worth while to give an account of how the vicarious transfer under consideration might conceivably take place.

Let us suppose that the reaction sequences represented in Fig. 10 correspond to the first and last members respectively of the habit-family hierarchy shown in Fig. 8, as in their original setting. The *S*'s represent external stimuli; the *R*'s, the gross reactions; the r_G's represent fractional anticipatory goal reactions; S_D, the persisting drive stimulus, *e.g.*, hunger; and the s's represent proprioceptive stimuli from preceding reactions. The final or goal reaction is represented as consisting of two components, R'_G and r_G. The latter is assumed usually to be relatively insignificant in absolute magnitude but, because of its supposed ability to move forward rather freely in the behavior sequence, to be of the greatest significance as the physical substance of goal or directive ideas. Solid arrows indicate what are presumed to be unlearned sequences, and broken lines terminating in arrows indicate tendencies to action which have been acquired by association or conditioning.

Suppose that the organism possessing the combination of excitatory tendencies shown in Fig. 10 should find itself in a somewhat novel situation presenting an initial external stimulus S_1', and a different drive (S_D') as well.[9] Suppose, further, that by trial-and-success the organism finds its way to the goal over the route involving the action sequence

[9] The deduction would be considerably easier if the original drive (S_D) were assumed in the new situation, but the outcome would be less general than to assume a distinct drive. It may easily be deduced from these and related considerations that, other things being equal, transfer should be distinctly more effective where the same drive (*e.g.* hunger) is involved in both the old and the new situations. This deduction may easily be checked by experiment.

R_1, R_2, R_3, R_4, R_5, R_6, R_7, R_G. Now, if this action sequence is repeated a few times, r_G will become conditioned to S_D' and so will be brought forward to the beginning of the series (**15**). By virtue of this fact, *r_G will be brought into the presence of S_1' and will therefore become conditioned to it* (Fig. 11). Accordingly, whenever the animal finds himself in the presence of S_1', particularly if there is a pause before the beginning of locomotion, S_1' as a stimulus will evoke r_G, which will bring with it the proprioceptive stimulus s_G. But s_G, by virtue of previous

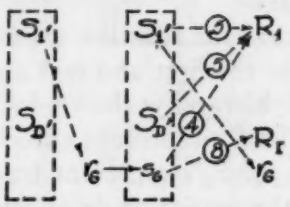

FIG. 11. Diagrammatic representation of the competition between the excitatory tendencies leading to R_1 and R_I upon the transfer of the habit system shown in Fig. 10 to a new situation permitting the same behavior adjustments but having different external stimuli and also a different drive (S_D'). S_1' indicates the initial external stimulus complex characteristic of the new situation. It is also assumed that the goal was attained the first few times exclusively by the longer path, *i.e.*, the one beginning with act R_1, and that during these repetitions incipient excitatory tendencies were set up leading to R_1 from S_1' and S_D'.

conditioning (Fig. 10), possesses excitatory tendencies both to R_1 and to R_I. Since, assuming a simple goal gradient, R_1 has at this point a strength of 4 units (Fig. 10) and R_I a strength of 8 units, this competition of excitatory tendencies will eventuate in reaction R_I, *which has never before taken place in this (objective) situation.* We accordingly have here a mechanism adequate to produce the transfer of practice effects which we have been seeking.[10]

Perhaps as good an experimental example of the concrete working out of this principle as any is found in a minor detail of a study by Johnson (**18**, 37). In this portion of Johnson's experiment, two dogs, blind from the first few days of life,

[10] It will be noted that the mechanism here indicated as mediating the transfer of habituation, while differing in important respects from that presumably responsible for the transfer in Shipley's conditioned reflex experiment, resembles it in that the element upon which the transfer was based is an *act.*

together with one normally seeing dog, were trained to open the door of a box containing food, by lifting a latch. The door of the box during the training period was always situated at the northwest corner of the box. After all three dogs had completed the learning in the original position, the box was turned 90° to the right. When the dogs had adjusted themselves to this position, the box was again turned to the right 90°. Thus the door was opened by the dogs successively at the N.W. corner, the N.E. corner, and the S.E. corner. One of Johnson's diagrams seems to indicate that the dogs always approached the box from the south. It was observed that when the door of the box was in the third or S.E. position the two blind dogs, even after 44 and 47 successive trials, always approached it by the roundabout way of the northwest and the northeast corners. This was the course by which the successive turnings of the box had originally led them. The seeing dog, on the other hand, took the direct route to the S.E. corner after the seventh trial.

The present hypothesis would explain these observations by supposing that in the free locomotion of the seeing dog there had been developed an effective hierarchy of locomotor habits in which the shorter path possessed a distinct preference. It required seven repetitions by the non-preferred member of the hierarchy to condition the anticipatory terminal reactions to the stimuli present at the point of choice. When this had taken place, however, the s_G at once evoked the dominant sequence of taking the shorter path to the S.E. corner, *i.e.*, practice effects acquired in a previous situation were first transferred to a non-preferred member of a simple hierarchy by means of practice and then automatically transferred to a preferred member of the hierarchy without any additional practice whatever in the latter act. It seems reasonable to suppose that the blind dogs, because of their sensory limitation, had not been able during their previous lives to build up such effective locomotor habit hierarchies as the seeing dog was able to do, which would account for their failure of transfer at this point in the experiment. However, these animals on still another turn of the

box to the right, which naturally brought about a still greater difference between the two paths to the door, succeeded rather tardily in making the transfer, which indicates that they possessed the habit-hierarchy mechanism at least to a limited degree.

V. The Habit-Family Hierarchy and the Principles of Frustration and Disinhibition

It must now be noted that the situation as represented in the preceding section will normally present certain significant complications not there discussed. In the process of conditioning S_1' to r_G, additional conditioned excitatory tendencies, which may be represented by $S_1' \rightarrow R_1$, would naturally arise. Now such excitatory tendencies must be in direct competition with R_I, and so are opposed to the transfer under consideration. If, however, the conditions of the experiment are such as to leave these excitatory tendencies fairly weak, *e.g.*, as shown in Fig. 11, the strengths of the respective excitatory tendencies might sum up as follows:

$$R_1 = 4 + .5 + .5 = 5,$$
$$R_I = 8.$$

Under these conditions the excitatory tendency to R_I would still be clearly dominant and the transfer would take place, though somewhat less promptly and with somewhat less certainty than without this complication.

If, on the other hand, a considerable amount of training should be given to the non-preferred member of the habit-family hierarchy under conditions such as that shown in Fig. 12, in which the preferred path is an integral part of the non-preferred path, as was the case in the experiments by Higginson, Gengerelli, and Valentine (36), a rather different situation arises. It is understood that during the main part of the training the door at *E* is always closed when the animals pass it on the way to *D*, but it is open when they return so that they can enter and go to the food at *F*. Under such conditions the excitatory tendencies emanating from S_1' and S_D' (see Fig. 11) may become fairly strong. Moreover, once

the long path has been traversed a number of times, transfer should take place to the preferred member of the hierarchy, at which time the animals will begin attempting to take the short path to *F* even though the door at *E* is closed. But under these circumstances we should expect to have a dominant excitatory tendency undergoing frustration, *i.e.*, being prevented from functioning. Under such conditions,

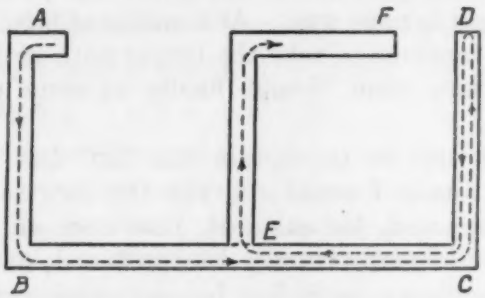

FIG. 12. Diagram of Valentine's maze (36). The broken line represents the required path of the rat during the early trials. The door at *E* was closed when the rat passed from *B* in the direction of *C*, but when he returned from *D* it was always open. *F* represents the point at which food was given.

there is reason to believe that an 'internal' inhibition will develop which is substantially like the experimental extinction observed in conditioned reactions.[11] We shall let the inhibitory tendency be represented by $a - 1$ and the two positive excitatory tendencies, one from S_1' and the other from S_D', each by a $+ 1$. With a moderate amount of training, then, the following situation might result:

$$R_1 = 4 + 1 + 1 = 6,$$
$$R_I = 8 - 1 = 7.$$

Even here the animal would still usually take the short path if the door were open on his way down from *B*. This actually took place in the experiments by Higginson (8) and Gengerelli (6).

But suppose that the training to go into the blind *D* (Fig. 12) were very great. Then one might expect the positive

[11] See Part II for an elaboration of this point.

tendencies to R_1 to be greater and the inhibitions of the tendency to go to R_1 to be more profound, in which case the excitatory tendencies might sum up as follows:

$$R_1 = 4 + 1 + 1 + .5 + .5 = 7,$$
$$R_1 = 8 - 4 = 4.$$

In such an event the animal would be expected to continue to follow the longer path unless the advantage possessed by it should be upset in some way. As a matter of fact, Valentine's animals did continue to take the longer path with fair regularity for a long time, though finally all came to take the shorter path.

But how are we to explain the fact that Valentine's animals all finally learned to take the shorter route? It might be supposed, for example, that once an animal has practiced the longer, naturally less preferred, path until the excitatory tendency to it has become dominant, he would always continue to follow it and would never thereafter eliminate the blind alley, succeeding practice serving to fixate the blind still more firmly rather than to eliminate it. This paradox, like so many that have arisen in the theory of learning, comes from assuming that only one principle, or at most a very few principles, are operating in the learning process at one time.

It happens that in this particular case, a principle from the conditioned reflex literature offers a ready explanation. It is well known (**25**, 44) that when an extra (disturbing) stimulus (or its after-effect) impinges on an excitatory tendency, the latter will be weakened; and when such a disturbing stimulus impinges on a situation where a previous excitatory tendency has suffered extinction, the inhibition becomes temporarily disinhibited, which enables at least a considerable portion of the excitatory tendency previously active to become active again (**25**, 49 *ff.*). We have already presented reasons for believing that there should arise such an inhibition of the tendency to go into alley *F* when coming down from *B*.

Let us suppose that we have one of Valentine's animals which is taking consistently the long path, *i.e.*, that the rat

goes from *A* to *B*, past the open door at *E* to *C*, then to *D*, back by *C*, through *E* and up to *F*. Suppose, now, that just before point *E* is reached (on the way to *C*) a loud buzzer is sounded. The excitatory tendency to *C* will be weakened according to the principle of *external inhibition*, whereas the inhibited tendency to go into *F* will suffer *disinhibition*, the latter reanimating the excitatory tendency to go directly to *F*. The respective tendencies which were originally as appear on p. 49 would be transformed somewhat as follows: [12]

$$R_1 = 4 + 1 + 1 + .5 + .5 = 7 - 2 = 5,$$
$$R_I = 8 - 4 = 4 + 2 = 6.$$

The excitatory tendency of R_1 which stood at 7 is weakened by two points through external inhibition, leaving 5, whereas the strength of R_I, previously weakened by experimental extinction from 8 to 4, recovers two points through disinhibition so that it now stands at 6. The tendency to R_I consequently becomes dominant. Therefore, after a disturbing stimulus, the animal might be expected to drop the blind into which he has been trained to go,[13] *i.e.*, to take the turn to the left even though previously he has always entered this alley by making a turn to the right.

A second deduction which is of considerable potentiality as an experimental test of the above hypothesis depends upon the well-known fact (**25**, 65; **32**) that external inhibitions and disinhibitions are notoriously transitory. This means that

[12] It is not yet clear from the experimental work on conditioned reflexes whether a disinhibiting stimulus affects only the inhibitory component or whether its effect is also upon the excitatory component as well. It seems reasonable to assume that the latter is the case. The net gain in strength of reaction in such a case would be due to the well-known fact (**25**, 66) that inhibitory tendencies suffer much greater external inhibition from stimuli of moderate strength than do excitatory tendencies. Thus the net gain in the strength of R_I of two points (from 4 to 6) might result from a combined reduction of the basic excitatory tendency of 8 to 7 through external inhibition and a gain in effective excitatory strength through a reduction by disinhibition of the four units of inhibition to 1. Thus the application of the results of conditioned reflex experiments to more complex situations give a unique significance to certain phases of conditioned reactions hitherto disregarded.

[13] Perhaps one of the most important biological functions performed by external inhibition (and disinhibition) is to prolong trial-and-error and prevent permanent fixation of maladaptive habit tendencies of which the long path in Valentine's experiment is an example.

under ordinary conditions a shortly succeeding test carried out like the above but without the buzzer should be about as likely to result in the entry of alley *D* as were those trials made previous to the trial with the buzzer, except for the positive conditioning influence of taking on this one occasion the shorter path from *A* to *F*.

The application of these principles to Valentine's experiment is obvious. Even under the most favorable experimental conditions, slight disturbing stimuli are constantly impinging on the experimental animal; they may come from the environment, or they may originate within the animal's own body. An obvious source of external inhibition would be the open door which the animal is not accustomed to see when on his way from *B*, especially if a strong light should be shining through it. Other sources of disturbance might be slight sounds, odors, the bites of parasites, etc. These disturbing stimuli should produce the combination of external inhibition and disinhibition exactly as described above, and thus bring about occasional choices of the shorter path.

Since external inhibition in its various forms is inherently transitory, such short-path choices are likely to be followed by runs on the long path, particularly if the interval between trials were as much as five minutes. Thus there might be expected to take place an irregular alternation between choices of short and long paths. Since it is probable that the experimenter would not be able to sense the majority of the stimuli disturbing the animal, particularly those of internal origin, these alternations would appear to ordinary observation to take place in an entirely fortuitous manner.

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[MS. received July 26, 1933]

THE PHYSIOLOGICAL BASIS OF LINGUISTIC DEVELOPMENT AND OF THE ONTOGENY OF MEANING.[1] PART I

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I. Infantile Crying and its Significance for the Adult. The Meaning of 'Significance'

The behaviour of the new-born human infant, during its waking hours, consists largely of reflex vocalizations and other random movements such as squirmings, writhings, and wrigglings. On account of its important rôle in the subsequent development of language, infantile crying, although it is regularly connected with other behaviour, has often been singled out for separate investigation. It is only by the artifice of abstraction that we can treat of sounds *as if* they existed separately from the other bodily manifestations concomitant with them. C. Bühler says, "The first and most striking observation to be noted in the first year is that crying *always occurs closely connected with movement.* We have, therefore, a unit of behaviour in which the crying is 'the dominant.'" [2]

The leading physiologists agree that the birth-cry of the human infant is a reflex response of the organism to various intraorganic and external stimuli. W. Preyer says, "The first cry is purely reflexive, like the croaking of the decapitated frog when the skin of his back is stroked. The cry is not heard by the newly-born himself and has not the least value as language. It is on a par with the squeaking of the pig just born, the bleating of the new born lamb, and the peeping of the chick that is breaking its shell." [3] And according to

[1] From the Department of Psychology of Princeton University.

[2] C. Bühler, The first year of life, 1930, 25. Italics in the original.

[3] W. Preyer, The mind of the child, Part II, 64–65. It must be pointed out that the auditory sensitivity of the new-born infant is still a subject of controversy among physiologists. Both Preyer and Stern, however, are in agreement with regard to the total deafness of many babies at birth. (See W. Stern, Psychology of early childhood, 1914, 74.)

W. N. Kellogg, "the reports of careful investigators indicate that the cry of the newly born orang-utan or chimpanzee is hardly distinguishable from that of the human infant." [4]

Charlotte Bühler remarks that "*All causes of crying in the first four months revert back directly to bodily hurts and needs.*" [5] And she specially mentions pain (*e.g.* colic, prick wounds, illness), strong and sudden stimuli (*e.g.* very bright light, sharp noises, sudden contacts, sudden changes of temperature), abrupt and sudden changes of posture, fatigue, hunger, etc. Margaret G. Blanton gives, as the principal causes, hunger, noxious stimuli, fatigue, and colic.[6]

Undoubtedly, crying implies no conscious meaning on the part of the infant during the earliest stages of infancy; but it has a meaning for adults, who understand it as a sign of the infant's distress. "A child's scream," says Jespersen, "is not uttered primarily as a means of conveying anything to others, and so far is not properly to be called speech. But if from the child's side a scream is not a way of telling anything, its elders may still read something into it and hurry to relieve the trouble." [7]

There is general agreement that along with the development of specific movements, there is a gradual differentiation of acoustic pattern in an infant's cries, according as they are evoked by this or that stimulus. M. G. Blanton writes: "The 'hunger cry' has generally a well marked rhythm, the first syllable of preliminary sound coming on the first part of the first beat, the second or accented syllable on the second part of the first beat, and a quick intake of breath as the third beat. This measure is most often repeated in groups of 5 or 6, each slightly more forceful than the preceding ones until the fourth or fifth, the last one being softer. Thus also will the groups be repeated. Each measure is also a trifle higher in pitch than the one preceding." [8] She describes as

[4] W. N. Kellogg, Humanizing the ape, PSYCHOL. REV., 1931, 38, 169.

[5] C. Bühler, The first year of life, 1930, 26.

[6] M. G. Blanton, The behaviour of the human infant during the first thirty days of life, PSYCHOL. REV., 1917, 24, 458–60.

[7] O. Jespersen, Language, its nature, development and origin, 1921.

[8] M. G. Blanton, *op. cit.*, 458–460.

follows the specific cry of colic: "Starting abruptly about 3 to 5 octaves above the adult female voice it slid through a modified chromatic scale to within the range of the middle octave. It was made with an accompanying rigidity of the abdominal walls and thus of necessity varied from the cry which included the activity of these muscles." [8]

D. R. Major observes that "The monotonous reflexive crying . . . soon gives way to cries which vary with the *kind* of discomfort the child is experiencing. . . . They [the cries] differ in pitch, duration, timbre." [9] C. Bühler also reports that cries develop specific configurations as the infant grows. Thus, in the first month "in addition to the monotone habitual crying" the new-born child "utters short cries of fright and single sounds of displeasure." [10] During the second month, she reports, the infantile crying ceases to be monotonous; "instead, it varies in intensity, rhythm and pauses." [10] During this period, according to her, different cries are associated with different bodily discomforts and needs. Thus, "The cry of hunger is continuous, loud, and strong and is interrupted suddenly by suckling movements, even if an objective stimulus does not come into direct contact with the lips." [11] The cry of pain, on the other hand, was observed to be "shrill, loud, and lasting, interrupted by whimpering and groaning, or it consists of short, single cries." [11] In the second month infantile crying is also accompanied with tears. She further reports that in the two-months-old child "habitual crying" may be distinguished from significant crying. The former is characterized by "uniformity, weakness, lack of variation in intensity, and interrupting pauses," and it continues "beyond an immediate need." [12] The accompanying bodily movements, she says, are not strong.

Still another observer, N. Bayley, reports in a recent study: "Fatigue crying was often accompanied by yawning, rubbing the eyes, or drooping." [13] Cry from colic pain,

[9] D. R. Major, First steps in mental growth, 1906, 284.

[10] C. Bühler, *op. cit.*, 35.

[11] C. Bühler, The first year of life, 1930, 36.

[12] *Ibid.*, 37.

[13] N. Bayley, Study of the crying of infants during mental and physical tests. *J. Genet. Psychol.*, 1932, **40**, 315.

according to her, was judged by its continuity, recurrence, and relief when gas was expelled. Crying evoked by strangeness of place and persons "usually occurred when the child was first brought in, or when he was taken from his mother for some of the tests. It was also evidenced by clinging to the mother and turning away from the experimenter. Crying from this cause stopped when the child became accustomed to the place, and when held by, or allowed to remain close to, his mother." [13]

To sum up: (1) Infantile crying is only a part of the total infantile behaviour at any given moment.

(2) It is purely reflex in nature; and is due to purely physiological causes. No intention to communicate, on the part of the infant, is involved.

(3) The adult learns to read meaning into the cry of the infant by a process of inference. Inside the child there is only released nervous energy (internal stimulation): from the motor effects of this released energy the adult *infers* (from his previous experience) this or that about the processes going on in the child's internal organs. A state of contentment and comfort is inferred when the infant's muscles are relaxed, and its body is in an attitude of repose.

(4) In order to interpret the infantile cry accurately it must be taken in conjunction with the entire behaviour of the infant, together with such stimuli as may be playing on it at the moment.

We come now to a point of importance. It is the question as to what, precisely, in an infant's movements, *constitutes* 'meaning.' The terms 'meaning,' 'significance,' and 'interpretation' need to be examined more closely. Why is it that a new-born infant's squirmings, writhings, and wrigglings, and even his early vocalizations, seem to be so lacking in significance? As we watch the movements of a very young baby we find ourselves saying that such mere writhing is pointless or aimless; the little limbs are flexing and unflexing at random; the organism is not doing anything in particular, not doing anything describable. Further observation of the infant, as those movements develop which we do

declare to be 'meaningful,' convinces us that 'aim' and 'point' come into the picture precisely when any of the baby's movements are aimed or pointed *at its environment.* The first change in the behaviour of the infant that gives us the impression that the infant's movements do have a 'meaning,' comes when we can see that they are directed towards (or away from) an outside object. If the child orients itself in *any* definite way towards a bright light, for instance, we instantly find the child's behaviour to be significant, meaningful. Nor can we easily resist inferring that the child is, to some extent at any rate, 'aware' of that light.

It is this fact of the orientation of organisms towards objects outside of themselves that leads us to find significance in 'tropistic' responses, at the lowest biological level. Inasmuch as the sunflower turns towards the sun, its reaction is significant. And the same principle holds in the case of the behaviour of more complex living organisms.

The question of the significance of an individual's behaviour, in short, reduces itself to the inquiry: How is the individual, infant or adult, orienting himself towards the objects around him? Or in other words, "What is the organism doing?" [14] This will involve, in E. B. Holt's words, "the study of his movements until we have found that object, situation, process (or perhaps merely that relation) of which his behaviour is a *constant function.*" [15]

Since the reaction of the organism to an objective situation must be something definite, we shall call it a *specific response.* It may be defined in the words of E. B. Holt as "a course of action which the living body executes or is prepared to execute with regard to some object or some fact of its environment." [16] Specific response is, therefore, to be sharply distinguished from various reflex movements which, however regular and fixed they may appear to be, do not have an intrinsic reference to anything located outside of the organism. To quote from Holt once more on this point:

[14] E. B. Holt, The Freudian wish, 1916, 168.
[15] *Ibid.*, 163.
[16] E. B. Holt, The Freudian wish, 56–57.

It is this objective reference of a process of release that is significant. The mere reflex does not refer to anything beyond itself: if it drives an organism in a certain direction, it is only as a rocket ignited at random shoots off in some direction, depending on how it happened to lie. But specific response is not merely in some random direction, it is *toward an object*, and if this object is moved, the responding organism changes its direction and still moves after it. And the objective reference is, that the organism is *moving with reference to some object or fact of the environment.* . . . In order to understand what the organism is doing, you will just *miss* the essential point if you look inside the organism. . . . It is the *pivotal outer object*, the object of specific response, which seems to me to have been overneglected.[17]

II. The Babbling Period, and the Mechanism of Reduplication

Observers agree that the random sounds first produced by an infant serve as the raw material for its later linguistic progress. Stern remarks that "even if the resulting 'echo-babble' is, at first, entirely incomprehensible, it yet enriches the learner's treasure with sounds, sound-combinations, rhythm, etc., the raw material then for future speech." [18] In fact, "phonetic experts tell us that the infant in his babbling covers the whole range of sounds utilized by Russian, Gaelic and Polynesian dialects." [19] K. C. Moore supports the main drift of this observation. She writes that "at the close of the fourth month my child had made well-nigh all the sounds that occur in the language." [20] According to C. H. Bean, during this period almost all the sounds of different languages may be heard in the babbling of the child. "The number of sounds that ultimately find a place in this self-imposed practice is astonishing. One cannot fail to hear all the vowels and consonants, diphthongs, aspirates, sub-vocals, nasals, German umlauts and tongue trills, French throaty trills and grunts, and even the Welsh *l*." [21] That many more

[17] *Ibid.*, 54–55.
[18] W. Stern, Psychology of early childhood, 1924, 141–142.
[19] F. Lorimer, Growth of reason, 1929, 33.
[20] K. C. Moore, The mental development of a child, *Psychol. Monog.*, 1896, 1, (No. 3), 115.
[21] C. H. Bean, *J. Genet. Psychol.*, 1932, 40, 198.

sounds are produced by the infant during this period than are later used, at least in its own language, is clear from the following remark of Preyer:

> According to my observations, I am compelled in spite of this disagreement to lay down the proposition as valid for all healthy children, that the greatly preponderating majority of the sounds the child makes use of after learning verbal language, and many other sounds besides these, are correctly formed by him within the first eight months, not intentionally, but just as much at random as any other utterance of sound not to be used later in speech, not appearing in any civilized language.[22]

Edward Conradi supports this observation:

> A great many sounds are produced during the babbling period which are not found in our language, some of them so complicated that we cannot produce them; among these are the so-called vomitive sounds and the cluck-like sounds, found among some primitive people but not in our own language.[23]

According to G. Stanley Hall, "It would be difficult even with Bell's phonic notation, or with a phonograph, to classify the first vocalizations of an infant." [24] A. Gesell states that "at the age of six months it requires diligent observation to record the wealth of vocalization which presents itself in the course of one day." [25]

Now, out of this astonishingly rich and varied repertoire of sounds, those which are used by the child's elders are reënforced, and become habitual; the others cease to be uttered. Preyer observes:

> The plasticity of the apparatus of speech in youth permits the production of a greater abundance of sounds and sound-combinations than is employed later, and not a single child has been observed who has, in accordance with the principle of the least effort (*principe du moindre effort*) applied by French authors to this province, advanced in regular sequence from the sounds articulated easily—*i.e.*

[22] W. Preyer, The mind of the child, Part II, 75–76.

[23] E. Conradi, Psychology and pathology of speech-development in the child, *Ped. Sem.*, 1904, 11, 328.

[24] G. S. Hall, Notes on the study of infants, *Ped. Sem.*, 1891, 1, 132.

[25] A. Gesell, Mental growth of the pre-school child, 1925, 213.

with less activity of will—to the physiologically difficult; rather does it hold good for all the children I have observed, and probably for all children that learn to speak, that many of the sounds uttered by them at the beginning, in the speechless season of infancy, without effort and then forgotten, have to be learned afresh at a later period, have to be painstakingly acquired by means of imitation.[26]

Almost as soon as sounds begin to be produced at all, they begin to be repeated, rather slowly and monotonously, one, two, three or more times. This *repetition*, or *reduplication*, may well be considered the first step in that process by which the mere vocalizations become organized into language. It is here that explanation of the development of language must begin. And it is here, precisely, that the ways part between most of the hitherto current efforts at explanation and that physiological description which alone seems to us to reveal the real causes that have brought languages into existence.

Some of the earliest reduplications observed are *a-a*, *ma-ma*, *da-da*, etc. Preyer holds that "These primitive syllables, *pa-pa*, *ma-ma*, *tata*, and *apa*, *ama*, *ata*, originate of themselves when in the expiration of breath the passage is stopped either by the lips (*p. m.*) or by the tongue (*d. t.*)." [27] But this can hardly be a sufficient explanation, since it is observed that the process often runs on to ten, twenty and even more repetitions, and further that it is far less in evidence in infants that are born deaf. There is clearly a definite mechanism that produces repetition. The phenomenon is so prominent during the months when the infant begins its vocalizations that this period of its life is called the 'babbling' period. And the tendency to reduplicate syllables (usually a consonant and vowel) is noticeable through the child's fifth year, at least, and to some extent through life.

Many explanations have been offered for the infant's repetition of his own sounds. So far as we have discovered, these explanations fall into three classes: the first would explain reduplication in terms of pleasure; the second, in

[26] W. Preyer, *op. cit.*, 76–77.

[27] W. Preyer, *op. cit.*, 86.

terms of heredity; and the third, in terms of instinct, the instinct of 'imitation.'

An explanation in terms of pleasure would deserve attention, if anyone had made clear how 'pleasure' can actuate the tiny nerves and muscles which we know to be the machinery that produces reduplication. No one has done this, and the 'explanation' does not explain.

We do not concede that an 'explanation' in terms of heredity (or of recapitulation) is even deserving of attention. For the question, 'How did language ever develop?' is not answered, or even met, by the reply 'It developed a *long time ago;* and is now inherited.'

And, lastly, if it is 'imitation' that constrains the infant to imitate his own vocal sounds, then it is 'imitation' which we propose to explain by means of the physiological mechanism called the 'reflex-circle.'

The principle of the reflex-circle was first formulated, we believe, by S. T. Bok.[28] E. B. Holt [29] has recently described how it works in the case of infantile babbling. We shall quote him at some length.

> The first results of the workings of Pavlov's law [of the conditioned reflex] are what Dr. S. T. Bok (1917) has called 'reflex-circles.' Let us consider *any* muscle *at a moment when* a nervous excitation, seeking some outlet of least resistance, purely fortuitously finds its way into the motor neurone of this muscle. The muscle contracts,—a random movement. But now something happens which is *not* random. Every muscle has sensory organs embedded within it, its proprioceptors, which are stimulated, probably by mechanical pressure, when the muscle contracts. And so the sensory cells of this muscle (of its tendons also and of the joint involved) are now stimulated and send an excitation along their afferent nerves to the central nervous system. But this excitation arrives only a second or two after the above-mentioned random

[28] S. T. Bok, The development of reflexes and reflex tracts, *Psychiatr. en Neurolog. Bladen*, 1917, 21, 281–303. Curiously enough, we find that a few writers, among whom are F. H. Allport and J. F. Dashiell, have made use of the principle of 'reflex-circles' *after* the appearance of Bok's article in 1917, without mentioning the original source. To the best of our knowledge Holt is the only writer who has acknowledged Bok's priority on this point.

[29] E. B. Holt, Animal drive, vol. I, 1931, 37–38.

impulse has found, or while it is still finding, outlet from the central nervous system into the muscle. Therefore by Pavlov's law (or equally by the law of neurobiotaxis) the incoming excitation will find outlet along the tract just used by the random impulses, that is, will go back to, and will further contract, the very muscle from which it came. Thus the afferent neurones from this muscle will begin to acquire, and after a few repetitions of this process will acquire, a synaptic connection with the motor nerve which goes out to this same muscle. A reflex-circle is established.

Now in the course of time what has happened to this (*any*) muscle, will happen to all the muscles: fortuitous impulses will sooner or later reach them all, and in all of them such a reflex-circle will then be established. The result is that in every muscle a contraction tends to reinforce and to perpetuate itself.

With regard to the application of this principle to the reiteration of infantile sounds, Holt writes:

It is well known that infants which are born deaf do not learn to speak, even though their vocal organs be absolutely normal (Preyer, 1889, pp. 42, 98). This is because a pre-requisite to the acquisition of speech is the establishment of reflex paths from the ears to the vocal organs, such that a sound received at the ears causes the vocal organs reflexly to reproduce that sound as closely as their anatomical structure permits. These indispensable reflexes are established inevitably if the infant's audition is normal. For, as is well known, its random murmuring, cooing, babbling, and other more strenuous vocalizations are, during certain of its early months, well-nigh incessant. It exercises *at random* its entire articulable gamut. Now each sound as it is produced stimulates the child's own auditory apparatus, if this is intact, and each such auditory excitation finds motor outlet in precisely that set of the vocal organs which has just made that very sound: and which will now make it again (a reflex-circle). Hence the infant's persistent reiteration of any sound which it has made (*cf.* Humphrey, 1921). So, little by little, if the child has normal hearing it becomes able to repeat also those same articulate sounds which *other persons* utter to it, and is well on the way to speaking. Of course, this principle is the basis of all onomatopoeia; and indeed Prof. J. M. Baldwin (1895, pp. 130–34) has called it the principle of 'simple imitation.' The reflex-circle in fact gives rise to a general law of iteration: A child will repeat any of its own random acts provided that this action (simultaneously)

stimulates, howsoever indirectly, any of its own sense-organs (and also of course, that no other reflex steps in to interrupt).[30]

Such, then, is the mechanism of the so-called imitative sounds. It is important to note that the circular activity involved in reduplicated vocalizations can be set in action by sounds other than those which the child himself makes. Thus a child in response to *ma*, uttered by the mother or the nurse, will say 'ma-ma-ma-ma,' etc. Prolonged crying—another case of reiterated sounds—may also be initiated either intra-organically or from without. The mechanism involved in this activity is exactly the same: the stimulus tends to reproduce or reinforce itself. G. Humphrey illustrates this case in the following words:

> Originally the stimulus was perhaps a pain, but as the child cries he hears himself crying. Then we have *S* producing *R*, and with it the auditory stimulus *S* 2, the sound of the baby's own cry in the baby's ear. Hence by the law of substitution of stimuli, *S* 2, the auditory stimulus, produces *R*, the reaction, and the more the baby cries the more he cries. Now it does not make any difference to the infant whether the auditory stimulus, once established, comes from himself or from another child. Whenever he hears the sound of crying he will cry, until the reflex has disappeared by 'lack of support' from the primary stimulus, that is, until he grows up and is not accustomed to hear himself crying. That is why the sound of a crying baby does not bring shrieks from a company of adults; the reflex has died out from lack of use.[31]

J. M. Baldwin appears to have been the first to use the expression 'circular-activity.' To quote him on this point:

> The reaction at which imitative suggestion aims is one which will *reproduce the stimulating impression*, and so tend to perpetuate itself. When a child strikes the combination required, he is never tired working it. H. found endless delight in putting the rubber on a pencil and off again, each act being a new stimulus to the eye. This is specially noticeable in children's early effort at speech. They react all wrong when they first attack a new word, but gradually get it moderately well, and then sound it over and over in

[30] E. B. Holt, Animal drive, vol. I, 1931, 39–40.

[31] G. Humphrey, Imitation and the conditioned reflex, *Ped. Sem.*, 1921, 28, 4.

endless monotony. The essential thing, then, is that *the stimulus starts a motor process which tends to reproduce the stimulus and, through it, the motor process again.* From the physiological side we have a circular activity—sensor, motor; sensor, motor: and from the psychological side we have a similar circle,—reality, image, movement; reality, image, movement, etc.[32]

Although Baldwin was early in discerning this principle, he did not have the full secret of the reflex-circle, and he unfortunately made use of the hedonic concept, *e.g.* 'delight,' which in addition to other defects involves the fallacy of enelicomorphism [33]—since this circular activity appears in early infancy. As Holt has said: "The ideas of reflex-circle and adient response have been used by Prof. J. M. Baldwin (1895, p. 133) but like Bain, Spencer, and most psychologists, he obscures the matter at once by hypotheses involving 'pleasure and pain.'" [34]

III. Origin and Growth of Meaning

Section 1. Meaning and Specific Response

The movements of any living animal are effected by energy which is stored in its (nerves and) muscles and released for action by either internal or external stimuli. There is a period in the development of the human fœtus when its movements, as described by Minkowski, are altogether "slow, unsymmetrical, arrhythmic, uncoördinated, diffuse and unformed." [35] This is because no definite functional connections have as yet been established in the fœtal central nervous system between the sensory and the motor nerves. By the

[32] J. M. Baldwin, Mental development in the child and the race, 1895, second edition, 132–133.

[33] We are indebted to Prof. Howard C. Warren for suggesting the term 'enelicomorphism,' (Gr. ἐνῆλικ- = mature), to denote the error which consists in interpreting infant behaviour after the analogy of adult experience. A view based on this fallacy may be, accordingly, designated as 'enelicomorphic.' This term seems to us rather more specific and less liable to ambiguity than either 'teleiomorphism' (E. Claparède, Experimental pedagogy, 1911, 92) or 'adultism' (M. Curti, Child psychology, 1930, 254).

[34] E. B. Holt, Animal drive, vol. I, 1931, 41 (foot-note).

[35] M. Minkowski, Sur les mouvements, les réflexes et les réactions musculaires du fœtus humain, etc., *Rev. Neur.*, 1921, 37, 1111.

time of birth a few such paths have been formed, the new-born infant shows some definite reflexes; but by far the larger part of its movements are still diffuse and uncoördinated.

At this time, as Stern has remarked, the infant gives no indication "of inborn ideas or of any real perception, i.e. seeing and noticing outside objects and occurrences as such, nor is there conscious will or endeavour. All that we are possibly justified in assuming is the presence of a dull, undefined foreshadowing of consciousness." . . .[36]

From birth on, the movements of the infant become more and more definite, and its random movements gradually subside, and fall into the background of the picture. The physiological aspect of this process has been described by Holt in Vol. I of his work called 'Animal Drive,' and we need not repeat the whole argument. The process is one of learning, which means histologically the growth of nerve dendrites according to C. U. Ariëns Kappers' law of neurobiotaxis and, from the point of view of behaviour, the steady conditioning of movements to stimuli as described by I. Pavlov. Considered broadly, the process is a progressive adaptation of the random movements of the organism to the obstacles which constitute its environment. It learns to go out 'adiently' (to use Holt's term) toward all ordinary stimuli, but to withdraw from stimuli which become over-strong. Thus it orients itself toward objects of the environment, navigates among them, explores them. It learns to take attitudes toward objects, to follow set courses of action with regard to them, and by exploring them to re-create their surface contours. All this is strictly physiological: a matter of movement, stimulation, and dendrite growth. The mechanism of imitation, described in the preceding section, illustrates the principles involved.

Now it seems to us that awareness of a stimulus is nothing else than this response that is made to it. Certainly the reaction of an organism is the principal if not the sole test of its awareness of any stimulus.[37] Indeed the response to a

[36] W. Stern, Psychology of early childhood, 1924, 75.

[37] *Cf.* W. Stern, *op. cit.*, 74–79.

stimulus is not merely the criterion of bare awareness, for over and above that the nature of the response reveals, in our opinion, what the stimulus has *meant* to the organism so responding. If, for instance, whenever a loud noise occurs near an infant, it quivers, puckers its lips, pauses in its breathing and then bursts into a loud wail, we can hardly resist concluding that the noise had some very vague 'meaning' for the infant; of the sort that we should call 'a disturbance,' perhaps a disagreeable disturbance. We should conclude this all the more confidently if a loud noise always elicited this same response. For what is it that ever assures us that another person has caught the meaning, even say, of a word if it is not that the particular word regularly elicits from the person a certain sort of response? If, on the other hand, a loud noise elicits from the infant sometimes laughter, sometimes a languid stirring of its limbs, sometimes screams, and sometimes no response at all, we are bound to conclude that the noise has no definite and settled meaning for it.

The position which we here adopt with respect to awareness and meaning is nothing new in psychology. The history of this view has been interestingly traced by Professor H. S. Langfeld;[38] and it is a long and impressive history although it has been systematically ignored by many academic psychologists. As Langfeld himself writes, "without response there would be no consciousness. No matter how many or how strong are the stimuli and the resulting afferent impulses, without the efferent impulses and specific response, either incipient or overt, an organism would have no awareness of a world; so far as that organism is concerned there would be no experience." [39] And again, "The meaning of an object is our attitude toward that object, our reaction to it." [40] Thus the infant's awareness of the meaning of a stimulus, and its motor response to the stimulus, are identical phenomena—two ways of describing the same process.

The same is true when the stimulus is a word. From the

[38] H. S. Langfeld, The historical development of response psychology, *Science*, 1933, 77, 243–250.

[39] *Ibid.*, 243.

[40] H. S. Langfeld, Concerning the image, PSYCHOL. REV., 1916, 23, 180.

infant's general reaction to the stimulus of mere noise to its more specifically differentiated reaction to a spoken word is only a further step involving essentially the same process of conditioning. Concerning primitive language B. Malinowski observes that, "A word *means* to a native the proper use of the thing for which it stands, exactly as an implement *means* something when it can be handled and means nothing when no active experience of it is at hand." [41] . . . "In all the child's experience, words *mean*, in so far as they act and not in so far as they make the child understand or apperceive." [42] Again, with Dr. Langfeld, "Words, in short, are conditioned responses to action, and only if we know this action, do we understand their meaning." [43]

When a mother assiduously ministers to her infant's needs, alleviates all irritations, etc., she produces in the infant a state of comfort, well-being and peace, which is not a condition of the child's sensory apparatus, but of his muscles—muscular relaxation, and calm. Now since it is the assiduous mother who effects this *motor pattern* in the infant, by removing sources of irritation and otherwise soothing him, she is regularly present and is seen and heard by the infant, *i.e.*, is as a visual and auditory *stimulus* a part of his total sensory pattern, at the moment when his motor pattern is this of relaxation and peace. Therefore, by Pavlov's law, the visual and auditory stimuli which reach the infant's eyes and ears from the mother, will produce in the infant a motor pattern of peace and well-being. By the same law *either* her visible aspect or her spoken 'Mamma' (with her peculiarities of intonation, etc.) will elicit in the infant the motor pattern of peace and well-being. Which is to say, that 'Mamma' as spoken by her 'means' to the child peace and well-being (a motor state).

If, again, the mother by affectionate and playful actions excites reflexes already formed in the child, such as tilting exuberantly back and forth in his rocking-chair, then (by

[41] B. Malinowski, The problem of meaning in primitive languages, in C. K. Ogden and I. A. Richards, The meaning of meaning, 1923, 488.

[42] *Ibid.*, 487.

[43] H. S. Langfeld, Consciousness and motor response, PSYCHOL. REV., 1927, 34, 8.

Pavlov's law, as before) her face and spoken 'Mamma' will come to 'mean' for the child a happy excitement—rocking back and forth in his chair.

An infant's awareness of some meaning does not imply any very precise appreciation. And, as a rule, the earlier meanings of an infant are exceedingly vague. Just as there are many grades of specificity between the random and the coordinated movements of a child, so also there are different degrees of precision in its meanings. Any individual's meanings, during the course of his life, will range from a vague and shadowy awareness to fairly clear and precise cognition. In short, whenever there is a response on the part of the organism to a stimulus, there is always meaning. But not until the response becomes specific can there be precise meaning. The explanation of the absence of precise meaning for an infant is the same as the explanation of his lack of coördinated and specific movements.

Section 2. Objective and Subjective Aspects of Response; Re-Creation through Empathy

When an organism responds specifically, the process presents two distinguishable aspects. Firstly, the response points out or is aimed at some part of the environment (even if this is only a position in space); it further may follow objects moving in space, and in general trace or explore (and thus in a sense re-create) the contours and relations of objects and their movements. Let us call this the objective reference, or aspect, of the specific response. Secondly, it is an organism with its motile members that is executing the response, and this implies posture or attitude, and changing attitude, of the organism. Let us call this the 'subjective' aspect, or reference, of the response. Clearly these two aspects are actually indissoluble.

If now the act of perception is an act of responding specifically, it is clearly the objective reference (re-creation) of the response which is the perception of the object, of its contour and other properties, and of its motion if it is undergoing change; while at the same time the inseparable subjective

aspect of the response is an attitude of the organism toward the perceived object, a doing something to it or with it. It is true that one or the other aspect may seem greatly to preponderate in a given response. The 'subjective' aspect, at any rate, can never be wholly absent; some instances suggest that objective reference may be so reduced as to be practically absent. But the two aspects are indissolubly fused in the actual response, and can be distinguished only by a conceptual analysis. In all acts of vision the objective reference preponderates; yet the attitude called 'interest,' or something of the sort, is inevitably there. While the hand that reaches for an object or that wields a knife is an instance primarily of 'subjective' attitude, yet the position of the object seized or the position, shape, and texture of the object cut cannot fail to be rather specifically defined by the motion of the hand, or knife.

When the stimulus responded to is a word or sentence, it is the response of the hearer which creates the 'meaning.' Wherefore the meanings of words have to be learned, even as the multiplication-table is learned. It is obvious that what logicians call the denotation and connotation of words are both realized in the objective reference of the response, while the 'feeling tone' or emotional value of the word lies in the subjective aspect of the response.

All evaluation is, of course, within the subjective aspect. The pleasant and unpleasant experiences of the child are its subjective or attitudinal responses to various stimuli. The responses of the new-born baby seem to be predominantly attitudinal or subjective.

To sum up: the meaning of a stimulus, whether an object present and perceived, or a word or other symbol which stimulates us to create a signification and to take an attitude, comprises in general the following items:

1. Objective reference:
    a. Perceived (or conceived) location of the object (denotation)
    b. Perception (or conception) of the properties of the object, what it is, etc. (connotation)

2. Subjective reference, or aspect:

Attitudes of acceptance, rejection, or other personal intention toward the object: feeling, emotion, evaluation, use.

It is the objective reference or process of perception (and the therewith implied process of motor, 'conceptual,' re-creation) that next calls for consideration. We believe that this process is one with that phenomenon which psychologists have called 'empathy'; that is, that perception is a process of re-creation through empathy.

Empathy,[44] or *Einfühlung*, was first brought into special prominence by Theodor Lipps, as the essential process underlying 'æsthetic perception.' It has been further studied and discussed largely by students of aesthetics: by K. Groos, Vernon Lee, C. Anstruther-Thompson, H. S. Langfeld, and many others. Aesthetic perception, as Prof. Langfeld has shown, is not a 'mentalistic' state, but a motor or empathic response of the percipient to the object perceived. Empathic response is the process through which 'the *activities* of the perceiving subject' are merged 'with the qualities of the perceived object.' [45] In other words, the percipient *re-creates* the various contours, lines and configurations of the object of æsthetic perception. But whatever the type of perception, æsthetic or other, the process of perception is the same. Thus, as Prof. Langfeld says, with regard to the perception of movement: "The perception of a movement . . . is first identified in the object and not in one's self." [46]

The idea is not new. Erasmus Darwin, writing in the year 1794, showed that what we should now call empathic response is essentially involved in the perception of an object.[47] And Alexander Bain has throughout his psychology contended that our perception of an object, of its size, solidity, form, shape, and distance are really motor activities on the part of the

[44] The word 'empathy' was coined by the late Prof. Titchener as an English equivalent for Lipps' term *Einfühlung*.

[45] Vernon Lee, The beautiful, 1913, 63.

[46] H. S. Langfeld, The æsthetic attitude, 1920, 115–116.

[47] Erasmus Darwin, Zoonomia; or the laws of organic life, (Dublin edition) 1794, vol. 1, sect. XXII, 3, 254.

percipient. Concerning the perception of objects and their location in space, he writes: "By movements confined to the head and eyes we grasp objects lying directly across the view, or with all parts equally distant from the eye; by these movements, combined with altered adaptation to distance, we have figures of objects that retreat from the view, as in looking down a street." [48] Concerning the perception of size and weight, he writes:

> By means of the movements of the eye, we acquire impressions of the *visual expanse or apparent magnitude*. This visual expanse of bodies is determined by the range or sweep of the eye in passing over their whole extent, or by the fraction of the field of view that they take in. We see a rainbow spanning one-third of the heavens; we see a cloud encircling the sky; we appreciate the dimensions of a picture on the wall as compared with other pictures beside it. The different degrees of movement and tension of the muscles that make the sweep are distinctly felt, and we set down one sweep as more or less than another. We also acquire by repetition standards of comparison for expanses in general, as we acquire standards of weight in the sensibility of the arms.[49]

With regard to the perception of solidity:

> A cubical block, exemplifying all the three dimensions of *solidity*, presents nothing radically new. A new direction is given to the hand, and a new class of muscles are brought to contribute to the feeling. The movement must now be over the length, over the breadth, and over the thickness, and the resulting impression will be a complication of the three movements.[50]

On the perception of form or shape:

> Form or shape is appreciated as easily as situation. It depends upon the course given to the movements in following the outline of a material body. Thus we acquire a movement corresponding to a straight line, to a ring, an oval, etc. This is purely muscular.[51]

On the perception of distance:

> For what do we mean when we say that an object is four yards distant from where we stand? I imagine that among other things

[48] Alexander Bain, The senses and the intellect, 1855 edition, 244-245.
[49] *Ibid.*, 245.
[50] *Ibid.*, 190.
[51] *Ibid.*, 193.

we understand this, namely, that it would take a certain number of paces to come up to it, or to reduce the distance from four yards to two yards. The possibility of a certain amount of locomotion is implied in the very idea of distance. . . . In the case of objects within reach of the hand, the movements of the arm give the measure of distance; they supply the accompanying fact that makes distance something more than an unknown visible impression.[52]

On the perception of empty space:

By the swing of the arms alone we have a notion of empty *space*, which means to us nothing more than scope for movement, and consequently for extended matter.[53]

Thus the perception of an object is to be described as the re-creation of objective configurations by copying movements executed by our motile members, including the 'lines of regard' of our two eyes. The following passage from E. B. Holt brings out clearly the rôle of movement in the general perception of space.

In his study of *The Psychology of Early Childhood*, Prof. Wm. Stern (1914) has entitled one section 'The Mastery of Space'. He there says (pp. 67–68), "The expression mastery of space signifies that not only the development of space-perceptions is to be discussed, but *the conquest of the spatial in cognition and in action at the same time*. The two are utterly inseparable; the structural unity of the sensory and motor comes out nowhere so clearly as in connection with space: there is no comprehension without intervention, no understanding without undertaking, no representation without a self-adjustment." Such a fact lends weight to the suggestion that the consciousness of space and action in space are the very same thing. In the terms of specific response that would mean that any specific response of the organism creates, i.e. *is*, the consciousness of that amount of space and of that position or configuration in space of which the response is a function: that is, which the response explicitly points to, defines, or re-creates (in the sense that a traced line 're-creates' the original over which it is traced).[54]

So far, we have described the process of the growth of

[52] *Ibid.*, 365.

[53] *Ibid.*, 192.

[54] E. B. Holt, Animal drive, vol. II. Quoted from the unpublished MS. by permission of the author.

meaning in the presence of the object. This process is at the perceptual level: it is perception. But the various stimuli that excite us are not always immediately present. An infant, for instance, reacts not only to the physically present mother or the nursing-bottle, but it also 'reacts to,' i.e. re-creates, these objects even when they are not in sight. But whether the stimulus is present or absent, our reaction to it involves the process of re-creation through empathic response. The difference is that in the case of the response to (re-creation of) an absent object, the original neuromuscular patterns must have been learned, so that they persist as "neurograms which, even when the original object is absent, will *re-create* its surface contour." [55] Erasmus Darwin described the re-creative process for ideas and thoughts (i.e. when the actual objects are not present), in the following words:

> It is hence, that we are very slow in acquiring our tangible ideas, and very slow in recollecting them; for if I now think of the tangible idea of a cube, that is, if I think of its figure, and of the solidity of every part of that figure, I must conceive myself as passing my fingers over it, and seem in some measure to feel the idea, as I formerly did the impression, at the ends of them, and am thus very slow in distinctly recollecting it.[56]

We saw at the end of the preceding section, how through the operation of Pavlov's law a tone or a word can serve as a substitute stimulus, a 'symbol,' for the physical object *mother;* so that in the absence of the mother it will touch off the same responses in the infant. The infant will thus re-create the familiar figure of its absent mother through the mediation of a symbol which stimulates its neurograms.

The response of the infant to the immediately present object is a perception. Its response to a symbol is a *meaning.* The process of re-creation has been sketched by E. B. Holt as follows:

> . . . the *meaning* of a word lies entirely in that which the word as a stimulus makes one *do*, even when such doing is performed at

[55] E. B. Holt, Animal drive, vol. 1, 1931, 216.

[56] Erasmus Darwin, *op. cit.*, vol. I, 110.

low tension and is invisible to one's neighbour; when, that is, it is only an inconspicuous play of muscle-tonus. This meaning will have objective validity, will be *cognition*, when and *in so far as* the movements in question are started, stopped, guided, and controlled by outer objects, that is, in mathematical parlance, when they are *functions* of outer objects. This much of the word-response is the word's objective meaning. The rest of the response which the word elicits is merely personal attitude (acceptance, rejection, liking, disliking, etc.), the personal element of the word's meaning. It is not surprising then that the objective or cognitive element of a word's meaning and the attitudinal or subjective element are so closely fused and so inseparable.

The meaning of a word, the response which it stimulates, as mental content and in detachment from the word or symbol itself, is what is called an 'idea' or 'concept.' And thus I believe that all concepts and ideas, in fact all mental contents, are actual motor responses; a part of the response, in each case, being a *re*-creation by motion of some configuration of objects in the environment—this the objective or cognitive part—and the rest of the response being merely a personal posture or attitude—this the subjective, emotional or affective element of the idea or concept.[57]

Section 3. Meaning of Gestures, Cries, and Tones

It is through the intervention of its elders that the general movements and postures of an infant gradually pass into symbolic gestures. A hungry infant, for instance, soon comes to react to the sight of the nursing-bottle by writhing, wriggling and directing its head and eyes towards it—general indications of food-adience. At the creeping stage an infant will creep towards its bottle. This is its direct response to the sight of the nursing-bottle. This response, however, has a 'significance' for the interested mother, inasmuch as she understands it to mean that the infant wants food. The general behaviour of the child may thus be termed a *whole-body* language, since the attitude and activity of its whole body convey meaning to an onlooker.

But this whole-body language of the infant soon comes to be abbreviated, through the solicitous participation of the

[57] From an unpublished paper 'On the conceptual affinity of opposites.' Quoted by permission of author.

mother. As soon as the infant shows a nascent attitude of food-adience, the mother brings the bottle and thus cuts short the adient efforts of the infant. Whenever it stretches out its hand towards an object, the attentive mother is there to put the object in its hand. Such coöperation on the part of the mother, soon reduces the whole-body language of the infant to mere ('conventional') gesture, in which *only a part* and that the earliest part of an action is substituted for the entire action. The responses of the infant thus become merely symptomatic, i.e. symbolic.

Dr. F. N. Scott describes how the infant's original motion of clutching, through social coöperation, passes over 'into the socialized, symbolic attitude or gesture which is called pointing,' as follows:

> In its original form this gesture is the act of seizing or clutching. Its primary purpose is the acquisition of food. Such a movement also serves as a recognition-sign, disclosing to others the presence, and to some extent the identity, of the individual making it, and also revealing his hungry condition. If a supply of food were always present, the act would never rise above its primitive stage. As often as the individual felt the need of food he would reach out his hand and take it. But the source of food supply, especially in the case of the very young, is not always within reach. The hand goes out towards it in vain. The stomach remains empty, and the futile clutching movement is merely a sign of increasing hunger.
>
> If, now, when such a movement is made by the child, the mother places the food within its reach, the clutching takes on a new aspect. The movement was at first completed by the act of the child; it is now completed by the act of the mother, since a part of the movement which was formerly made by the child alone, is supplied by the mother's movement. This vicarious completion of the child's act has important consequences. . . . The effect of this is to abort or abbreviate the movement, for since part of it is to be performed by another, the whole movement need not be made. All that is necessary is a reaching toward the object; the mother will do the rest. Hence, in course of time, the grasping part of the movement will be abandoned. The clutch will be shortened into a mere thrust of the hand accompanied by an expectant look at the mother. The life-serving movement of clutching will have passed over into the gesture of pointing.[58]

[58] F. N. Scott, The genesis of speech, *Publ. Mod. Lang. Ass. Amer.*, 1908, 23, xxxiv–xxxvi.

All this shows (1) how a whole-body activity of the infant is abbreviated into a gesture, and (2) why even the vestigial gesture has a meaning *for the mother* (and often for her alone).

Now this cutting down of the infant's original adient efforts toward food, to mere gesture, provides the infant also with a 'meaning' connected with his gesture. The mother by solicitously proffering the bottle on seeing his incipient striving for food (his 'gesture'), produces in the child the following sequence of reflexes; and these, after sufficient repetition of the meal-time experience, become connected as a chain reflex:[59] internal hunger stimuli (appetite)—gestures of food-adience; [the mother sees and steps in] mother as visual stimulus and nursing-bottle as visual, tactile and olfactory stimuli—motor exploration (perception) of mother and bottle, and motor appropriation of the bottle and its contents.

For the first few times, the advent of the mother with the bottles is a necessary link in the sequence of reflexes. But as in all learning of chain-reflexes, after some repetitions the proprioceptive afferent impulses returning to the central nervous system from one movement have become canalized into the motor paths of the next following movement, and are sufficient of themselves to produce the movement. From this moment on, the mother herself is not an actually necessary link in the chain. On the contrary, if the mother does not see the infant's gesture and bring the bottle, the infant's next movements will be *as before*, to re-create its mother and to grasp and carry to its lips the (not-present) bottle. And precisely this it is which we have all often seen, and said: "The baby is definitely *looking for* his mother; he *wants* her to bring the bottle." Such is the physiological mechanism of 'expectation' and 'purposive' action in general. This 'expectant' re-creation of its absent mother and grasping reach for the absent bottle are the gesture's *meaning* in the experience of the infant. They mean, translated into words: I want mother bring bottle.

[59] 'Agglutinated' is the term used by Alexander Bain, The senses and the intellect, 1855 edition, 322–328. William James used the term 'concatenated': *cf.* W. James, Principles of psychology, vol. I, 115.

While the present writer was studying this physiological aspect of expectation, a friend chanced to have the following experience with a dog. Walking down the street, Mr. A saw on a lawn a large dog that he had never previously seen. The dog was looking toward A, and A in passing smiled and made a friendly gesture to the dog. With a slight wag of his tail the dog turned about, ran to the back of the lawn and returned, still running, and carrying in his mouth a rubber ball, which he dropped squarely in front of A's feet. A stopped, and the dog looked up at A 'expectantly,' wagging his tail. A threw the ball, and the dog bounded off to fetch it back; and the game was on. On his second return, and thereafter, the dog dropped the ball into A's hand.

It is a game which a dog is at all times ready and eager (motor-set) to play. The master's part and the dog's part alternate, the dog's turn beginning when the master throws the ball and ending when the dog returns the ball to his master's hand. But if the master does not throw it again, the dog's next turn cannot be carried out, and is reduced to 'expectation.' When from the lawn the dog saw A walking past, all the *stimuli* were present which were needed to set the dog to playing the game. With alacrity he played his turn; and laid the ball at the feet of an utter stranger. This act was a 'gesture,' and as typical an instance of 'meaningful gesture' as could well be conceived. It can scarcely be denied that the dog made the gesture with 'a definite purpose in his mind.' Nor can it reasonably be denied that the physiological mechanism of the performance was a well established chain reflex.

The dog dropping the ball at A's feet is completely analogous to an infant waking up in its crib, hungry, and crying 'in order to' summon its mother with the nursing-bottle.

The child's understanding of the behaviour of others does not begin with its appreciation of words; it begins with an appreciation of its elders' actions and gestures. As Colonel Mallery writes:

> It [the child] learns words only as they are taught, and learns them through the medium of signs which are not expressly taught.

Long after familiarity with speech, it consults the gestures and facial expressions of its parents and nurses as if seeking thus to translate or explain their words.[60]

And Stern observes:

Gesture has its value too, in those speaking to children, as a help to the understanding of many words, for the meaning of many gestures is at once apparent to the child; when these are the constant accompaniments of certain words, at last the words alone are sufficient to convey the meaning. If at the words, "Well, come then," a child learns to get up and struggle along to his mother, it is because these words have been accompanied by the highly suggestive gesture of outstretched arms.[61]

That a few words, or any other prominent stimulus, out of the many simultaneous stimuli present in a total situation should elicit from the child (practically) the same response as does the whole situation, merely illustrates that 'part-for-the-whole association' which is a fundamental property of the conditioned response principle. The phenomenon bears witness to the underlying physiological mechanism. A word does this neither more nor less than does any other significant stimulus or gesture which the organism has previously encountered. In quite the same way, Wolf, the favourite dog of Rip Van Winkle, who had learned by frequent painful experiences to remember Dame Van Winkle's thrashings, found meaning and significance in the mere sight of her threatening attitude; for, in the words of Washington Irving, "at the least flourish of a broomstick or ladle, he would fly to the door with yelping precipitation." [62]

That the significant use of the gesture has preceded speech also in the linguistic development of the human race, is obvious from its copious, and in some instances its exclusive, use among primitive peoples. Prof. G. F. Stout reports that "It is well established that the Bubis of the island of Fernando

[60] G. Mallery, Sign language among North American Indians, in First Annual Rep. Bur. Ethn. Smithsonian Instit., 1881, vol. I, 276.

[61] W. Stern, Psychology of early childhood, 1924, 145.

[62] W. Irving, The sketch book, in The works of Washington Irving, 1856, vol. II, 47.

Po cannot understand each other in the dark. Miss Kingsley in her 'Travels in West Africa' tells us that among the Fans it is common to propose to go to the fire in order to see what people are saying." [63] Even where speech appears to be more or less developed, in a primitive society, gestures are still employed to convey the more precise meaning of the speaker. Lord Monboddo writes: "the savages in North America do at this day supply the defect of their language by a great deal of action and gesticulation." [64]

The process of abbreviation of the whole-body activity into the symbolic gesture is also obvious in the case of primitive man. Ethnologists report that the movements of the entire body, involved in the communications of the primitive man, are generally replaced by the movements of the hands. Thus James Mooney observes, concerning the American Indians:

> The signs are made almost entirely with the hands, either one or both. . . . Thus the sign for man is made by throwing out the hand, back outward, with index finger extended upward. . . . Woman is indicated by sweeping downward movement of the hand at the side of the head, with fingers extended toward the hair to denote long flowing hair or the combing of flowing locks.[65]

The use of hands is again illustrated in his account of the gestures denoting more abstract concepts:

> Thus, *fatigue* is shown by downward and outward sweep of the two hands is front of the body, index finger extended, giving a gesture-picture of utter collapse. *Bad* is indicated by a motion of throwing away.[66]

As vocal language (tonal or verbal) develops, the gesture-language recedes into the background. But not even at the highest levels of vocal communication, do gestures ever entirely disappear. They persist in a more or less vestigial form. For even when a speaker makes no conspicuous gestures, slight movements are always to be detected: movements of the

[63] G. F. Stout, A manual of psychology, 1899, 475.

[64] Lord Monboddo (James Burnet), Of the origin and progress of language, vol. I, second edition, Edinburgh, 1774.

[65] J. Mooney, Sign language, Bureau Amer. Ethn., Bull. no. 30, 567.

[66] J. Mooney, Sign language, *ibid.*, Bull. no. 30, 568.

eyes and eye-brows, and of the general facial musculature. In many persons, indeed, the facial musculature exhibits a continuous 'play of emotion,' as it is called. Thus Colonel Mallery writes:

> Even among the gesture-hating English, when they are aroused from torpidity of manner, the hands are involuntarily clapped in approbation, rubbed with delight, wrung in distress, raised in astonishment, and waved in triumph. The fingers are snapped for contempt, the forefinger is vibrated to reprove or threaten, and the fist shaken in defiance. The brow is contracted with displeasure, and the eyes winked to show connivance. The shoulders are shrugged to express disbelief or repugnance, the eyebrows elevated with surprise, the lips bitten in vexation and thrust out in sullenness or displeasure, while a higher degree of anger is shown by a stamp of the foot.[67]

This persisting accompaniment of gesture is so considerable that Diderot, for instance, deemed that even in the most mature use of (spoken) language, gesture and intonation together continue to express actually more of a speaker's meaning than do his vocabulary and diction, however accomplished these latter may be. And this opinion coloured Diderot's views on the art of acting. F. Luitz says in his introductory Notice to Diderot's 'Le Paradoxe sur le Comédien': [68]

> What a large place he [Diderot] wishes to see accorded to pantomime in a dramatic work. He feels that there is too much speaking and too little acting, in the tragedies. . . . For the actor should move us rather by his actions and by his tone of voice, than by his words.

Long before an infant can understand words, it makes considerable advance in the language of tones. Thus M. M. Nice reports that though her daughter's vocabulary, at two years, consisted of five words, yet the child was able to communicate 'to a large extent by tones of voice and gestures.' [69] And this is true of most children. Of the wild boy

[67] G. Mallery, *op. cit.*, 279–280.

[68] In the Bibliotheca Romanica, (Heitz, Strasbourg) No's 179–182, 7.

[69] M. M. Nice, A child who would not talk, *Ped. Sem.*, 1925, 32, 107.

of Aveyron, Itard writes:

> Victor owes to it [the sense of hearing] the fact that he can distinguish words of one syllable, and above all, when presented with different intonations of the language, can recognize with considerable accuracy those which are the expressions of reproach, anger, sadness, contempt and sympathy.[70]

The process of conditioned response is in evidence here. Thus Major writes:

> The caressing sounds get their meanings perhaps from being frequently associated with gentle and tender handling, and harsh sounds come to have a certain response because associated with rough, or indifferently gentle handling, and not because in either case the distinction is untaught and instinctive. . . . If a child were accustomed to hearing soft words and being beaten at the same time, such words would soon come to arouse fear and trembling. If, on the other hand, gentle and caressing treatment were associated in the experience of the child with harsh sounds, the latter would soon come to be soothing and quieting.[71]

The same is eminently true of animals. Charles Darwin recorded that:

> Monkeys soon learn to distinguish, not only the tones of voice of their masters, but the expression of their faces, as is asserted by a careful observer. Dogs well know the difference between caressing and threatening gestures or tones; and they seem to recognize a compassionate tone. . . . This limited amount of knowledge has probably been gained, both by monkeys and dogs, through their associating harsh or kind treatment with our actions; and the knowledge certainly is not instinctive. Children, no doubt, would soon learn the movements of expression in their elders in the same manner as animals learn those of man.[72]

Herbert Spencer believed that intonation is a part of what we have called the 'subjective' aspect of meaning.

> All speech is compounded of two elements, the words and the tones in which they are uttered—the signs of ideas and the signs of

[70] J. M. Itard, The wild boy of Aveyron, 1932, 60.
[71] D. R. Major, First steps in mental growth, 1906, 286.
[72] C. Darwin, The expression of the emotions in man and animals, 1872, 358.

feelings. While certain articulations express the thought, certain modulations express the more or less of pain or pleasure which the thought gives. Using the word *cadence* in an unusually extended sense, as comprehending all variations of voice, we may say that *cadence is the commentary of the emotions upon the propositions of the intellect.* This duality of spoken language, though not formally recognized, is recognized in practice by every one; and every one knows that very often more weight attaches to the tone than to the words. Daily experience supplies cases in which the same sentence of disapproval will be understood as meaning little or meaning much, according to the vocal inflections which accompany it; and daily experience supplies still more striking cases in which words and tones are in direct contradiction—the first expressing consent, while the last express reluctance; and the last being believed rather than the first.[73]

Romanes held similar views, declaring "that words are never understood unless tones are likewise so, and that understanding of words may be assisted by understanding of the tones in which they are uttered." Further, "while the understanding of certain tones of the human voice extends at least through the entire vertebrated series, and occurs in infants only a few weeks old; the understanding of words without the assistance of tones appears to occur only in a few of the higher mammalia, and first dawns in the growing child during the second year." [74]

Finally, in at least some primitive languages and even in so highly developed a language as the Chinese, intonation is a recognized means for conveying meaning. Using "*accent* in the antient sense of the word, to signify a musical modulation of the voice, by which it is made higher or lower with respect to gravity or acuteness," Lord Monboddo writes, "I believe all the antient and original languages, without exception, have a great deal of accent or tone in them." [75] He continues:

The Chinese language . . . is full of tones, insomuch that sometimes the same monosyllable signifies nine or ten different

[73] H. Spencer, Essays scientific, political, and speculative, vol. II, 1899. The origin and function of music, 421–422.

[74] G. J. Romanes, Mental evolution in man, 1893, 124.

[75] Lord Monboddo (James Burnet), Of the origin and progress of language, vol. I, second edition, Edinburgh, 1774, 470.

things, according to its different accents. The Indians too in North America . . . have tones by which they make the same word signify different things; and particularly one of those nations, the Hurons, according to the account given us of their language by Gabriel Sagard, . . . supply the defects of their language, particularly the want of tenses, persons, numbers, and genders, by accents only. . . . The Jesuit Sebastian Rasles, who was missionary in Canada in 1723, says the same thing of the Huron language; for he tells us, that the same word has different significations according as it is differently accented.[76]

[76] *Ibid.*, 471–472.

[MS. received July 12, 1933]

THEORETICAL STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY

TONAL FUSION

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Forty-three years ago Stumpf published the second volume of his 'Tonpsychologie.' The first volume, dealing chiefly with the problems and methodology of auditory sensation, appeared in 1883 and was followed by an interval of seven years before its author could bring together the numerous observations which form the point of departure for the second volume. "Ich hätte nicht gedacht," writes Stumpf apologetically, "dass es so lange dauern würde. Ich hätte es wirklich nicht gedacht." He reminds the reader, however, that not even a German professor of philosophy can be expected to go about all the time with reeds, pipes, and tuning forks.

Improvement of apparatus and refinement of observations have now reduced Stumpf's first volume to a work chiefly of historical interest, with the exception of those parts treating of psychophysical methodology and the psychological factors which influence the judgment of tone. Stumpf may well be interested in the degree to which physicists have occupied themselves with the problems of auditory sensation, for in a later publication in which he defended the systematic views of his teacher, Brentano, he argued that there can not be a psychology of tone, and was therefore forced to state, in anticipation of the question as to what he was doing in his own famous investigations, that he was not concerned with the phenomena of tone as such at all![1]

[1] "Was ich 'Tonpsychologie' nannte, sollte keineswegs eine Phänomenologie der Töne sein, sondern eine 'Beschreibung der psychische Funktionen, welche durch Töne angeregt werden.' . . . Eine Psychologie der Töne kann es eben nicht geben, nur eine solche der Tonwahrnehmungen, Tonurteile, Tongefühle." C. Stumpf, Zur Einteilung der Wissenschaften, 1907, 30, footnote.

In spite of Stumpf's insistence that he was having to do with psychical functions, not with neutral prescientific sensory phenomena, his fame rests not upon his dialectic contributions to the psychology of act, but upon his investigations of what would unhesitatingly be called sensation or perception. The second volume rapidly became classic, and few studies of auditory perception since his time have failed to take serious account of the wealth of material there reported. It would be tedious to cite the texts and articles in psychology which have referred to Stumpf as the final authority with respect to the degrees and laws of fusion. During recent years, however, certain investigations have appeared which make it necessary to quote Stumpf with greater caution.

The definition of fusion itself has always been troublesome. The difficulty began with the ambiguity which Stumpf himself introduced by using at least half a dozen different definitions. If two tones whose frequencies stand in the ratio 1 : 2 are sounded together, (1) 'so können sie nur sehr unvollkommen gesondert werden' as compared with two tones whose frequencies form the ratio 40 : 77 (**12**, II, 127). Fusion is nevertheless (2) 'eine unveränderliche Eigentümlichkeit des Empfindungsmateriales' which persists when all other obstacles to analysis have been removed (**12**, II, 127). It is that special relation of sensory contents (3) 'wonach sie nicht eine blosse Summe sondern ein Ganzes bilden,' (**12**, II, 128) as well as that peculiar relation as a result of which the total impression (4) 'immer mehr dem Einer Empfindung nähert' (**12**, II, 127). This last type of relation is realized more fully (5) 'je konsonanter das Intervall ist' (**13**, 35). What this all amounts to, writes Stumpf almost in despair (6) 'muss man eben hören und kann es Niemand klarmachen, der nicht hören oder Gehörserscheinungen nicht beobachten kann' (**13**, 44). He concludes by summarily dismissing all difficulties with a 'Solvitur audiendo.'

According to Stumpf the degrees of tonal fusion fall into five main classes:

First the fusion of the octave (1 : 2);

Second that of the fifth (2 : 3);

Third the fourth (3 : 4);
Fourth the natural thirds and sixths (4 : 5, 5 : 6, 3 : 5, 5 : 8);
Fifth all remaining musical and non-musical tonal combinations.

Stumpf uses musical terminology for the intervals not because of any musical significance which he attaches to them, but merely in order to have convenient and well-known names for the numerical ratios of the frequencies. In the sections on fusion he is dealing with tonal phenomena, not musical experiences.

Subsequent investigators have not been content to include several intervals together in the fourth and fifth classes, but have sought to rank these intervals too with respect to degree of fusion. Almost all texts, quoting one set of results or another, follow this practice. Titchener, *e.g.*, lists the intervals in the following order: 1 : 2, 2 : 3, 3 : 4, 4 : 5, 5 : 6, 3 : 5, 5 : 8, 5 : 7, 4 : 7, 8 : 9, 15 : 16, 8 : 15, and 9 : 16 (**16**, 351).

A source of no small concern to those interested in tonal fusion has been the obstinacy with which the intervals refuse to stay put in any given rank order. The octave practically always comes out in first place. The fifth, however, by no means always comes second. It may fall below the sixths and thirds. The fourth frequently goes ahead of the fifth, and at other times drops into the lowest class. The sevenths and seconds not only interchange positions among themselves; they sometimes move into the higher classes, especially the subminor seventh (4 : 7), as Stumpf himself admits. The reason for this is to be found partly in the equivocality of the concept of fusion itself, but more especially in the fact, which will be dwelt upon in more detail presently, that all intervals, with the exception of the octave, no matter what the frequency-ratio, are nearly indistinguishable with respect to the criterion which probably comes nearest to Stumpf's or anyone else's notion of fusion, *viz.*, unitariness of tonal impression.

In 1913 Kemp, working mainly with questions of consonance and dissonance from the point of view of music, found that observers could judge intervals on the basis of four distinctly different criteria: (1) fusion or *unitariness* as com-

prehended by the Stumpfian definition, 'eine unveränderliche Eigentümlichkeit welche noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden,' (2) *sinnlicher Wohlklang*, (3) *sinnliches Zusammenpassen*, and (4) *harmonisches Zusammenpassen* (**5**). The rank orders, as determined by the different criteria, were not the same.

Several years later the present writer performed a series of experiments which amply justified the assumption that the concept of fusion would turn out to be no less equivocal if it were approached from a more strictly psychological rather than musical angle (**10**). Unitariness of tonal impression seemed to be the criterion which gave the closest approximation to the Stumpfian classes of intervals, although the statistical differences between the positions of the fourth, sixths, thirds, tritone, and minor seventh were small and the differences between observers sometimes large. The observers moreover occasionally balked at the criterion and insisted that it was of little help to them in discovering differences which they could report with any feeling of assurance. Certain other criteria, of which there were no fewer than ten, yielded statistically more reliable results while establishing bizarre rank orders of intervals. The average rank order from the criterion of unitariness was: 1 : 2, 2 : 3, 3 : 4, 5 : 8, 3 : 5, 4 : 5, 5 : 6, 9 : 16, 32 : 45 (tritone, practically identical with what Titchener calls subminor fifth, 5 : 7), 8 : 15, 8 : 9, and 15 : 16. If this order is compared with Titchener's given above, and if the relative positions of the intervals belonging to Stumpf's fourth and fifth classes in the two orders are correlated, the result is 0.63. The correlations from the other criteria would be even lower.

In 1924 Brues performed further experiments on fusion. He had been impressed not only by the general lack of significant differences between most of the intervals, but also in particular by the relatively high positions which the sevenths often secured for themselves. He noted furthermore that for the criterion of *smooth-rough* which the present writer had used, the sevenths were nearly as high as the fifth, fourth, and sixths, and the minor third only a little above the seconds (**10**,

497). It seemed reasonable therefore to suppose two things: first, that the low position of the seconds and even the minor third might be due to the roughness caused by beats, and that the higher position of the sevenths might be brought about by the less pronounced beating of a fundamental with a second partial; and second, that all other intervals, with the exception of the octave, whether of simple or complex ratios, whether musical or non-musical, might reveal about the same degree of unitariness (1). In the middle region of the scale, where most of the work on fusion has been done, a major second with *C* of 256 as the root would have 32 beats. A minor third, with 51 beats, might have a trace of roughness. The 57 beats of a minor seventh (fundamental and second partial) produced by the fairly pure tones of Stern variators would scarcely be audible.

Brues used the twelve natural musical intervals, and the twelve intervals which lie geometrically half way between each adjacent pair of tempered musical intervals. The latter all have complex ratios. In terms of cents [2] the intervals were: 50, 112, 150, 204, 250, 316, 350, 386, 450, 498, 550, 590, 650, 702, 750, 814, 850, 884, 950, 996, 1050, 1088, 1150, and 1200. These intervals were presented a number of times to observers for judgments in terms of unitariness on an absolute scale. The scores thus obtained are represented by the solid line in Fig. I. They have been changed into per cent of distance along the absolute scale in order to make them more comparable with the scores, represented by the dotted line, secured by Guthrie and Morrill (4). These investigators, working quite independently, secured very similar results by a different method and with a larger number of observers. They also used a shorter range of frequencies and a larger number of intermediate intervals. Only those intervals are represented in the dotted curve which coincide, or so nearly coincide that the difference is negligible, with those used by Brues. *N* and *J* in the figure refer to *minor* and *major*. The curve is for judgments of consonance, which was defined, however, as purity, smoothness, and good blending: a good enough ap-

[2] A cent is 1/1200 of an octave.

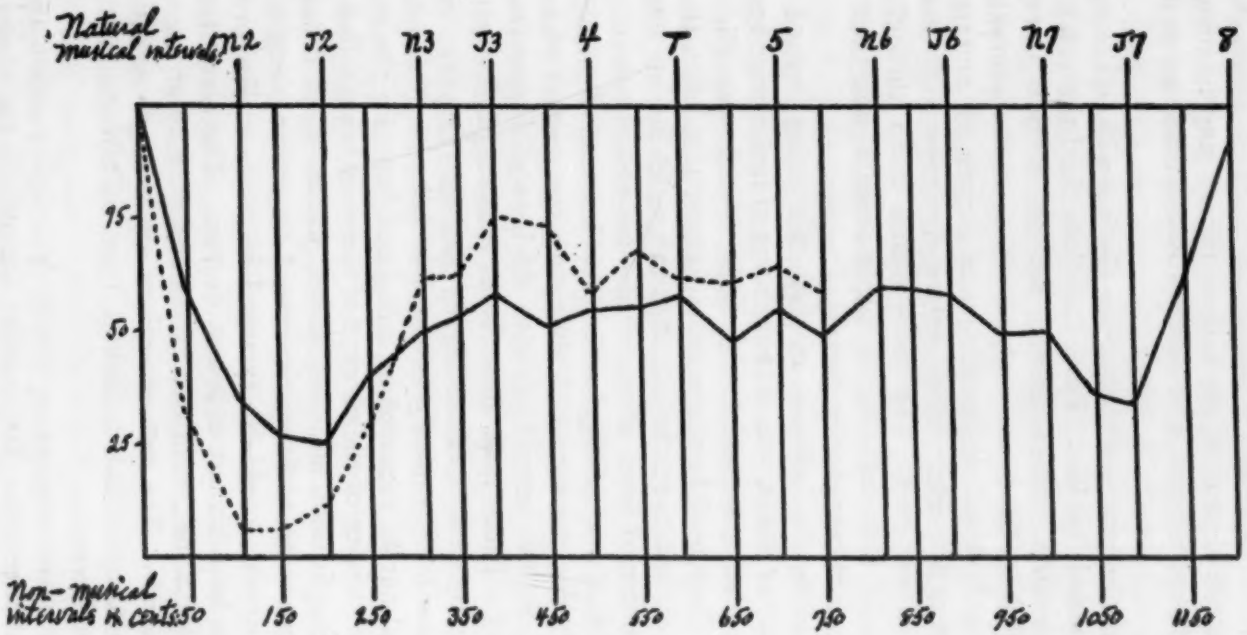
Natural musical intervals
n2
J2
n3
J3
4
T
5
n6
J6
n7
J7
8
95
50
25
Non-musical intervals in cents
50
150
250
350
450
550
650
750
850
950
1050
1150

FIG. 1.

proximation to whatever Stumpfian fusion is, although one which, for some reason, the authors think 'may be revolting to many psychologists, but which proved satisfactory to naive *Os*.'

It will be seen at a glance that the intervals in the region of the seconds are low. From the minor third through to the minor seventh (fifteen musical and non-musical intervals, some of which have very complex ratios) all the intervals are higher and reveal about the same degree of unitariness. *Throughout this whole middle region significant differences in fusion do not exist.* The curve comes down through the intervals of 1050 cents to the major seventh, and then rises through 1150 cents to the octave.

These results indicate at least one thing beyond any possibility of doubt, viz., if the concept of fusion were applied to these results, it could in no way be related to simplicity of ratio. They tend furthermore to suggest that, with the exception of the octave, the only intervals which deviate from an undifferentiated level of unitariness are those in which the roughness of beats disfigures the tonal impression.

Following the section dealing with the degrees of fusion Stumpf devotes several pages to the laws of fusion (**12**, II, 135–140). These laws have been restated and discussed critically by various writers (**6**, 285–304; **15**, 333–337). Since these laws for the most part stand or fall with fusion itself, they will not be repeated or considered here, for the reason that the validity of the concept of fusion is being called into question. Stumpf intended fusion, whatever its definition might be, as an auditory perceptual dimension along which tonal intervals could be arranged in more or less orderly sequence from highest to lowest degrees. The above results demonstrate that, with two exceptions, *such an arrangement is impossible.* The two exceptions are the octave and the somewhat artifactual deviations from unitariness resulting from roughness.

The octave occupies an absolutely unique position among all tonal intervals. It possesses unitariness far above the average level of all other intervals and, conversely, its two

pitch-components can be separated out under attentive analysis only with considerable difficulty. No other interval is like it in these respects. The other exception relates to roughness. The seconds are considerably below the average level, but if the seconds could be sounded without beats, it is very likely that they would be judged fully as unitary as the other intervals. Observers tend to identify roughness with auditory complexity, lack of unity (**10**, 501). The sevenths, in which the beats are much less pronounced than in the seconds, are judged as only slightly less unitary than the fifth or sixths.[3]

The difference between tonal intervals, let it be said again, can not be described or indicated in terms of a perceptual dimension of fusion. Fusion therefore as an explanatory or descriptive concept has no relevance or value for audition. Neither can these same intervals be described or differentiated in terms of any of the simpler sensory dimensions. These dimensions, in ordinary psychophysical procedure, are used as instructional limitations which enable an observer to report the difference between two psychological events with respect to some given aspect, as when he is told to report whether two colors are qualitatively the same or different, whether one is brighter than the other, more extended than the other, lasts longer than the other, or is clearer than the other. These questions, if put to an observer with respect to intervals, are meaningless, with the possible exception in certain cases of protensity and attensity. Intervals do not have *a* pitch or *a* volume, neither do they differ from one another intensively or extensively. They have qualitative differences, to be sure. A minor sixth is very different from a major third, but these differences can not be arranged in any sort of continuum. They are unique. Does this mean the endless multiplication of qualitative dimensions?

The danger of such multiplication is more apparent than real. The results and interpretations of recent experimental investigations in the field of perception have sensitized psychologists to the difficulty, if not impossibility, of dealing

[3] Experiments are now being made to determine more accurately the position of the sevenths in relation to other intervals.

adequately with sensory organizations in terms of simple attributive units or elements. These same organizations seem also not to fall into well delineated continua of a higher or perceptual order. They nevertheless do fall into typical groupings or classes. Tonal intervals constitute an excellent example of just such a class. The psychological task is merely to find out *what the conditions are* which produce any typical event in such a class. A description of the event itself, either in strict terminology or in beautiful words of inspiration and intuition, is of little use to science. Careful use of certain descriptive rubrics may offer preliminary knowledge of some importance, intuitions may delight the poet and philosopher; but the task of science is more far reaching and at the same time more prosaic. Let the event be whatever the poet or the man of common sense or the highly trained introspectionist says it is. Can psychology discover the conditions which make the event whatever it is said to be? If fortunately enough the event shows some decent sort of correlation with the physical dimensions of the stimulus, the task is already half completed, and the danger of multiplying psychological dimensions vanishes, for the simple reason that no new psychological dimensions need be introduced. Stimulus correlation, not psychological description, is the thing.

To anyone who can recognize intervals at all, a given interval is not an auditory experience with this or that degree of clearness of the parts, this or that degree of unitariness. It is a unique quality which defies verbal description. And yet an interval is easily identified, although fusion is probably the last way, with the possible exception of the octave, in which such identification is accomplished. Many other ways are simpler and more reliable,[4]—none more so than the usual device of assigning a name to the interval. The word 'fusion' itself is an unhappy one for psychology of today. The Oxford dictionary defines it as 'the union or blending together of

[4] For an excellent account of the way observers can identify intervals by aspects wholly unrelated to fusion, see E. M. Edmonds and M. E. Smith, The phenomenological description of musical intervals, *Amer. J. Psychol.*, 1923, 34, 287–291.

different things as if by melting; the result or state of being so blended.' The indescribable character of an interval by which it is so readily identified is not, when listened to as such, made up of fusing or blending parts at all. To be sure, a person can hear out the parts, but when he does so he no longer hears the original interval-quality. For psychology it is not important to describe the quality. Let it simply be called a fourth or a minor seventh, and let it go at that.

For a long time the approximate physical conditions for intervals have been known. Intervals are determined roughly by the ratios sustained by the frequencies. But only roughly. Just as pitch remains constant through small changes of frequency, so intervals remain unaltered through small changes of ratio. There is an interval-limen just as there is a pitch-limen, and the one can not be deduced from the other. According to recent research the pitch-limen at 40 decibels between 500 and 4000 cycles is about 5 cents.[5] The limen for interval-discrimination is very much larger. If observers, using the method of average error, are asked to alter the variable member of a pair of tones until a given interval is reached, the *m.v.* of the average settings for all intervals is about 20 cents (**8**). This value must not of course be regarded as final. The judgment is not an easy one to make, the method used undoubtedly has an important bearing on the outcome, and individual differences among observers may not be negligible. More recent investigations with an entirely different method and with a larger number of observers but with only three or four intervals, gave values over 30 cents (**9**). Even this discrepancy, however, is not nearly so large as those which have appeared in studies of pitch-discrimination.

The physical correlate, then, of an interval is not *a* ratio, any more than the physical correlate of a pitch is *a* frequency. Intervals and pitches both have thresholds, ranges of variability. But these ranges of variability are rigidly limited and can be stated, in terms of the stimulus, with a fair degree of

[5] H. Fletcher, Speech and hearing, 1929, 152. The older results which used to be quoted so frequently gave values as low as 1 cent! See W. Preyer, Ueber die Grenzen der Tonwahrnehmung, 1876, and E. Luft, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Phil. Stud.*, 1888, 4, 511–540.

precision. No question need be raised as to the *psychological nature* of an interval, except as one asks what the conditions are which produce the interval. The physical conditions for pitches and intervals are known. The physiological conditions are much less accessible, but certainly not beyond reach, as newer techniques of inquiry have amply demonstrated. When the physical and neural conditions of any psychological event, such as a tonal interval, are all enumerated, the event itself, for purposes of science, is thereby sufficiently described.

Summary

1. The concept of fusion, as defined by Stumpf, is equivocal.

2. The rank order of tonal intervals, arranged according to degrees of fusion, has never been satisfactorily established.

3. The octave occupies an absolutely unique position among all intervals.

4. With the exception of the octave and those few intervals whose auditory surface is disturbed by roughness, all intervals, whether their ratios are simple or complex, have approximately the same degree of unitariness.

5. The concept of fusion has therefore no significance for auditory phenomena.

6. For scientific purposes any psychological phenomenon, like an interval, is adequately described and accounted for as soon as its physical and physiological conditions are stated.

7. The physical conditions for tonal intervals are already known.

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[MS. received October 5, 1933]

CONSTRUCTIVE REACTIONISM KNOWLEDGE OF REALITY FROM A PSYCHO-PHYSIOLOGICAL VIEWPOINT

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From the atomistic philosophy of ancient Greece we have inherited two scientific principles of dubious value. One is general in its applications. The other is more specific. The general principle is that reality may be adequately known by particularizing analysis. The more specific is that the external world is known primarily by such parts of it as enter the mind through the gateways of receptor sense organs.

Both principles have aroused criticism and been repudiated from time to time in the development of the several sciences and of philosophy. Both, however, have shown great vitality, probably due to their apparent congruity with common sense and both have important consequences in contemporary scientific thinking. Both have influenced the development of other sciences than the science of psychology, but it is their influence on psychology that I would now especially consider.

The Primacy of Analysis

Analytical or particularizing description of events yields what is probably fundamental correlation. Whole-part relationships represent one of the earliest forms of systematization. They are obvious preconditions of classification and generalization. Of course analysis is complete only when it reaches indivisible units, but any stage is a contribution to knowledge. General psychology cannot afford to neglect it.

Under the influence of enthusiasm for other forms of systematization in psychology, it has recently become a fad with some eminent psychologists to disparage the findings of analytical description even when they were obtained under rigid experimental conditions. The real fault of the psycho-

logical analysts, it seems to me, lies not in a fallacious method, but in the assumption that theirs is the only scientific description of mental phenomena. It is probably as great a mistake to claim exclusive validity for configurated systems as for psychological atoms. In the anatomy of knowledge there are many correlation systems. We know no wholes without parts—no parts without configuration. But wholes are never merely coexistent fragments. Every known whole involves some principle of integration which particularizing analysis cannot reach. To discover and describe the dynamic conditions of biological and physical systematizations is one of the great tasks of scientific exploration and adventure.

The Immigration of Reality

It is, however, the second of the dubious principles of Greek atomism that has exercised the most pernicious influence on the development of psychology; the doctrine that a real external world somehow enters mind through the gateways of receptor organs. This most persistent fallacy of common sense deserves scientific reexamination. There are negative and positive considerations.

Negatively there seems to be nothing in that particular form of integration which we call 'mind' that comes from without the individual living organism. "The monads have no windows," as Leibnitz picturesquely expressed it. Positively it may be reexamined in connection with one of the best founded principles of general physiology. Every activity of the living organism must be regarded as belonging to a dual process of self-regulated metabolism and reaction to stimuli. Possibly both sides of this dual process may ultimately come to be regarded as different phases of a unitary event. There is some experimental evidence that this is true, but it is as yet inconclusive. Anabolic reactions of the various tissues to the presence of available food supply, and katabolic reactions to super-threshold changes in the vital environment remain the ultimate sources of all organic activity. Stimulus and reaction are correlates. Each implies the other. But no stimulus migrates into the reacting tissue. It only evokes

reactions in specific tissues in so far as these tissues have inner capacities for reaction and an available store of potential energy which is ready to be transmuted into kinetic energy. Hence the difference between a living organism and a dead one. In organic activities, action and reaction are neither equal nor necessarily in opposite directions.

Implications of Constructive Reactionism

The implications of this principle of general physiology are wide spread and important. They range from epistemology to psychiatry, from psychology to the theory and structure of science, from penology and education to ethics and religion. Let us present in the present abstract some of the implications of the principle of constructive reactionism in the structure of our knowledge of the external world.

The first epistemological consequence of constructive reactionism is the negation of both realism and idealism. Knowledge neither comes from without the individual, nor arises within him except under the control of external stimuli. As reacting mechanism the organism has no windows through which knowledge can come. Mind as an organic phenomenon is consequently not furnished by outside agencies. It is not a *tabula rasa* inscribed with foreign characters, but a self-contained system, a phenomenon of a reacting monad. There is consequently no probability of identity between knowledge as reaction and reality as external stimulus. Sensations must be regarded as the primary reactions of the neural organism to specific stimuli, operating on areas of specialized irritability. As such they have no validity, and give no immediate or intrinsic knowledge of the external world. Red is not vibration frequency, but an organic reaction to it. The quantification, spatial and temporal arrangement and the logical systematization of sensations are dependent upon inner constructive capacities. However, only pragmatically confirmed constructs can be true. The several systematizing and confirming capacities may be deranged or deficient, or they may be trained to a high degree of efficiency. Even they give no guarantee of identity of knowledge with an external world.

They may be pragmatically satisfactory, but for systems and configurations the reacting organism obviously has no avenues of immigration. We know of no neural mechanism by which an external field of force could be transmuted into an inner psychophysiological field—as Levin does so casually. All mental life is direct or indirect reaction.

Any actual systematization of primary and secondary reactions into a satisfactory view of a real world outside of us is a complex organic achievement. It may be grossly inadequate and perverted. It can never be either complete or perfect. The adequacy of any view is a pragmatic concept and changes with the nature of the demands made upon it. Scientific adequacy is very different from that of naïve common sense, though it is probably only a development out of it. However adequacy is defined, that view of the world is most nearly adequate in the organic sense which meets in experience a minimum of barriers or inhibitions and a maximum of agreements or facilitations.

The Validation of Constructs

Every so-called fact is a construct. As fact it must be congruent with an individual's reaction system whether it be a naïve fact of common sense or a scientific fact. Only in the latter the criteria of congruence are more rigid and the validating systems are more extensive. The only psychophysiological device for testing the validity of a construct is whether, and in how far, it meets inner or outer barriers. Inner barriers are incongruencies between a construct and experiential systems, as when a hypothesis is opposed by past experience. Outer barriers are extra-organic resistances to organic reaction, as when a hypothesis meets experimental difficulties. Barriers, either inner or outer, may be insuperable and lead to the abandonment of a hypothesis; or partial and lead to its correction. The former case conforms to the principle of trial and error, the latter to the principle of approximation. These barriers should be regarded not as handicaps to knowledge, but rather as invaluable conditions of adequate mental systematization.

Barriers to primary or sensory reactions are relatively inconspicuous and infrequent. One may on occasion doubt the adequacy of a sensation if it is incongruent with systematized past experience or with another sensation. Illusions and hallucinations are examples. But confidence in the validity of sensory reactions is naïvely insistent and almost irresistible. The ordinary man trusts them implicitly. They are his prototypes of truth. When man learned to distrust his senses it implied strongly reinforced systematizations such as those of science. All new systematizations are of the general nature of hypotheses. They are subject to inhibitions by inner and outer barriers and to the reinforcement of congruent persistent systematizations. Strong hypotheses are those that have not met apparently insuperable barriers and are more or less reinforced. A hypothesis is discredited by any insuperable inner or outer barrier. It is validated or verified when in addition to meeting no barriers it is reinforced by persistent systems. Scientifically the device for verification is the experiment, which at its best should be so arranged as to disclose any possible barrier. If the chances are very great that possible barriers would have been disclosed by experiment, while no barriers are met, the hypothesis is regarded as scientifically proven. Such hypotheses yield predictions of high probability in our hazardous environment. Hypotheses are practically validated by congruence with available persistent systems, when they meet no insuperable barriers in the ordinary conduct of life. Such pragmatic validation gives opportunity for all sorts of fallacies which it is one of the main functions of science to correct. When inner and outer barriers are persistently ignored or unrecognized we call such fallacies delusions. They frequently occur when current hypotheses are emotionally reinforced or when critical acumen is deteriorated.

[MS. received September 28, 1933]

DISCUSSION

IS EMOTION A MERE TERM OF CONVENIENCE?

In a paper by D. A. Worcester, 'In Defense of the Whale—Emotion is at Least a Term of Convenience,'[1] it is argued that emotion is a term which should not be abandoned because it refers to certain bodily conditions which justify study. It should be pointed out that abandonment of a term does not imply abandonment of the study of phenomena loosely referred to by that term. Psychologists who no longer use the term 'instinct' as an explanatory concept nevertheless study types of behavior formerly subsumed under that category. And if we should cease now to employ the present loose concept of emotion, we should not thereby cease to study visceral changes or changes in the intensity and coordination of other reactions frequently mentioned in descriptions of 'emotion.' But we should study these phenomena in their own right and under precise labels that do not mean different things on different occasions or to different writers. Moreover, we should not have pre-judged the situation by the employment of a term implying a sharp division in phenomena where none has in fact been shown to exist.

Worcester points a parallel between the use of the concept 'emotion' and the use of such classifications as cold and hot water which, he maintains, have aided in advancing our knowledge in spite of the fact that no sharp line can be drawn between the two categories. We welcome the parallel but dissent from the conclusion. 'Hot' and 'cold' are terms of convenience for the layman, just as 'emotion' may be said to be, but the scientist cannot profitably make use of such loose categories in his investigations. The physicist will prefer to speak of *gradations in temperature*, and thus he will avoid two pitfalls which psychologists have not succeeded in escaping in certain of their classifications: (1) the implication of a sharp division where none exists, and (2) the corollary assumption that these categories operate as entities in causal sequences. For example, the physicist does not assume that 'hotness' will produce one effect and 'coldness' another, but rather that the effect produced will depend upon the *degree of temperature*. If physicists used

[1] D. A. Worcester, In defense of the whale—emotion is at least a term of convenience, PSYCHOL. REV., 1933, 40, 478–480.

'hot' and 'cold' as we use 'emotional' and 'non-emotional,' we should find them attempting to correlate changes in the volume of various substances with 'hotness' and 'coldness,' though they could not say where one began and the other ended.

Worcester finds a further reason for preserving the term 'emotion' in the fact that, though every element present in emotion may be found in other conditions, "the arrangements of these elements and the intensity with which they function may result in conditions sufficiently different from each other that it will be a matter of at least scientific convenience to designate them by different terms." It may well be that some day in the future we shall be able to describe in exact terms a pattern of reaction different in kind from other patterns and to this give the name 'emotion'; but our contention is that we have not as yet done so, and that, when we invoke this hypothetical condition as an explanatory concept, we have no more explained the phenomena in question than when we invoke the mysterious force of 'instinct' to account for behavior the mechanism of which we cannot describe. Until we can name the 'elements' to which Worcester refers and can describe the particular 'arrangement' and 'intensity' of these elements which will constitute 'emotion,' we are hardly justified in using the concept of emotion in exact investigation. It appears to the writer that we shall make more rapid progress toward the exact description of such a unique condition, if indeed it exists, if we abandon the attempt to deal as such with that ill-defined complex of phenomena called 'emotion' and bend our energies toward the investigation of variations in certain specific aspects of behavior, such as the intensity of reactions or the degree of disorganization in behavior. With more complete knowledge we may be able to describe complex patterns of behavior different in kind from other patterns of behavior. Whether one of these patterns shall be called 'emotion' is of little consequence. But it is important that we shall not at the present time attempt to use a concept which has been given no discriminable content.[2]

[2] The writer is publishing a more detailed discussion of the concept of emotion in the March, 1934 issue of this journal.

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[MS. received October 3, 1933]

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